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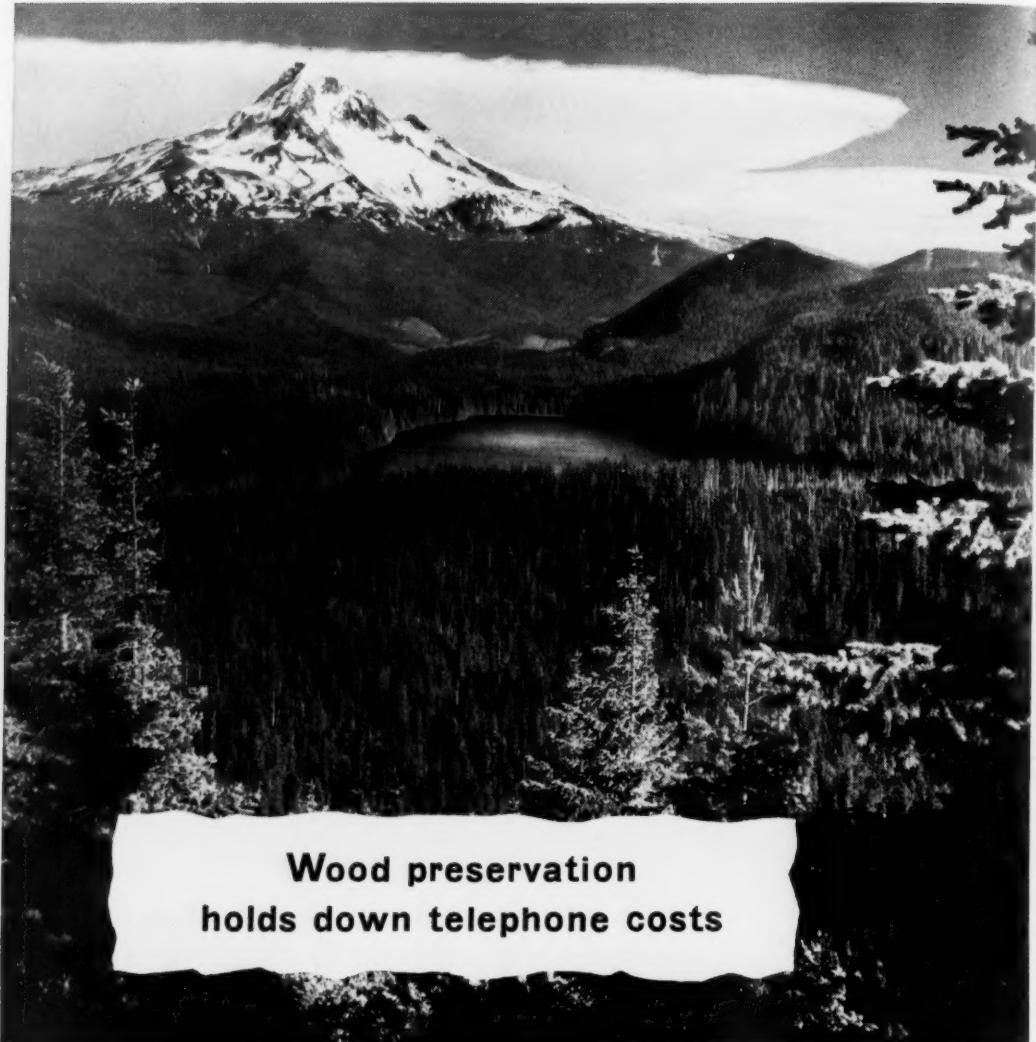
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Wood preservation holds down telephone costs

Poles are a substantial part of the plant that serves your telephone; making them last longer keeps down repairs and renewals that are part of telephone costs. So Bell Laboratories have long been active in the attack on wood-destroying fungi, the worst enemies of telephone poles.

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THE SCIENTIFIC MONTHLY

VOL. LXX

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NO. 5

❖ Special Book Issue ❖

Terrace Agriculture in the Colca Valley, Peru Cover
(Photo from Shipee-Johnson Expedition, courtesy Victor W. von Hagen)

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Science and Technology

House Organs

Some of the most beautiful and interesting scientific magazines being published today are the house organs of various industrial laboratories and similar organizations. Listed below are some of those that have come to our attention this year.

Bakelite in Review (quarterly). Bakelite Division of Union Carbide and Carbon Corporation.

Bell Telephone Laboratories Record (monthly).

Bell Telephone Magazine (quarterly). American Telephone & Telegraph Company.

Cenco News Chats (quarterly). Central Scientific Company.

Dow Diamond (bimonthly). The Dow Chemical Company.

Dupont Magazine (bimonthly). E. I. du Pont de Nemours.

The Educational Focus (bimonthly). Bausch & Lomb Optical Co.

Ethyl News (monthly). Ethyl Corporation.

The Flying Red Horse (bimonthly). Socony-Vacuum Oil Company.

Frontier (quarterly). Armour Research Foundation.

Inco (quarterly). International Nickel Company.

The Kellogram. The M. W. Kellogg Company.

The Laboratory. Fisher Scientific Company.

The Lamp (five times a year). Standard Oil Company.

Long Lines (monthly). American Telephone and Telegraph Company.

The Martin Star (monthly). The Glenn L. Martin Company.

The Merck Report (quarterly). Merck & Co.

The Orange Disc (bimonthly). The Gulf Companies.

Progress Thru Research (quarterly). General Mills.

Steelways (bimonthly). American Iron and Steel Institute.

Westinghouse Engineer (bimonthly). Westinghouse Electric Corporation.

What's New. Abbott Laboratories.

New Pamphlets and Catalogs

Railroad 'Rithmetric, Books I and II, edited by Olive W. Dennis and published by the B & O, Baltimore, Md., presents, by means of mathematical problems, many interesting aspects of railroad operation. For use in elementary and junior high schools.

Glass Blowing Equipment is a new 48-page catalog (No. 50), recently issued by the Scientific Glass Apparatus Company, Inc., 49 Ackerman Street, Bloomfield, N. J.

Bulletin No. 238 of the Council for Scientific and Industrial Research (Australia) is *The Non-parasitic Disorders of Apple Fruits in Australia*, by W. M. Crane. Published in 1948, it has just reached this country from the Government Printer.

The Fish and Wildlife Service of the U. S. Department of the Interior has issued a Special Scientific Report entitled *Translations of Foreign Literature Concerning Lobster Culture and the Early Life History of the Lobster*.

It is issued in limited quantities for the official use of Federal, state, or cooperating agencies.

The 1950 catalog (80 pp.) of Science Research Associates lists six categories of services: Tests, guidance publications and services, classroom texts, student learning aids, professional books, and business and personnel materials. Address: 228 S. Wabash Ave., Chicago 4, Ill.

Tracerlab's new *Catalog B* contains 90 pages. It may be secured from Tracerlab Inc., 130 High St., Boston 10, Mass.

Militarism in Education, the 80-page report recently issued by a group of twenty-six eminent authors and educators, may be obtained at 25 cents per copy, or 5 copies for \$1.00, from the National Council Against Conscription, 1013 18th St., N. W., Washington 6, D. C.

The Institute of International Education marks its thirtieth anniversary by the publication of *Blueprint for Understanding*. A limited number of copies are available from the Institute, 2 W. 45th St., NYC 19.

In conjunction with the Association of American Colleges, the Citizenship Clearing House has prepared a survey and report on the *Evaluation of Citizenship Training in American Colleges and Universities*, the result of a study of 218 representative colleges and universities. It is aimed at stimulating participation in public affairs at the local level.

Available from the U. S. Government Printing Office, Washington 25, D. C., at 20 cents, is a pamphlet on *High School Staff and Size of School*, by Ellsworth Tompkins and Walter H. Gaumnitz.

General Electric's new, revised *Manual of Electric Instruments* describes the fundamentals of construction and operating principles of all major types of electric instruments. \$1.00 per copy from the company at Schenectady 5, N. Y.

A list of the most significant books published in France on geology, paleontology, and allied sciences between 1940 and 1948—the *French Bibliographical Digest*—can be obtained free of charge from the Cultural Division of the French Embassy, 934 Fifth Ave., N. Y. The first three issues, containing bibliographies of French works on biology and geography, are also available.

Ultra-Microtomy by a New Method, by Sanford B. Newman, Emil Boyrsko, and Max Swerdlow, a publication of the National Bureau of Standards, is available at ten cents per copy from the Government Printing Office.

Maps

The New York State Department of Public Works and the Geological Survey have announced the completion of large-scale mapping of Niagara Falls and vicinity. The new map is on a scale of 1:24,000, with relative elevations of the land above sea level shown by contour lines at 10-foot intervals.

Index maps of each state and of Alaska, Hawaii, and Puerto Rico, showing areas covered by topographic maps, may be obtained free from the Director, U. S. Geological Survey, Washington 25, D. C.

THE SCIENTIFIC MONTHLY

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The Kinsey Report and its Contributions to Related Fields

The Kinsey Report and the Law

MORRIS L. ERNST

This article and the four following ones, and the Comments by Professor George P. Murdock, are based on a symposium held during the 1949 Meeting of the AAAS in New York City last December. Mr. Ernst, the distinguished lawyer, author, and lecturer, has been a member of the New York law firm of Greenbaum, Wolff & Ernst since 1915.

ANTECEDENT to reference to the Kinsey contribution to the law, I want to say a word about law in general—a concept as bewildering to scientists as science is to lawyers. Law is the study of unique situations; each case is different from another, and the imponderables lie not only in the drives and desires of men but in the far less definable standards constantly created by society—standards so vague that a juvenile delinquent may be called a boy who disappoints society either by his behavior or by getting caught. But, as in all groups engaged in a research magnificent, the frustration of the research may well appear in etymological semantics, or in the unconscious attempt to hold power over the laity by taboos, by Latin phrases, or by prescriptions set forth in undecipherable writing.

We must remember that there are two parts to law—one, the finding of the facts in a particular case and, two, the application of the societal rule to be applied to such facts in any particular culture

at any particular moment of history. Whereas law may never be a science, there is little valid reason why law should not use science as a tool—either in the resolution of conflicts between man and man, or between man and societal desires.

The present soft spot in the law lies in the finding of the facts—since few “law facts” can be tested by any known litmus paper. Facts in law are first refracted in the eyes of the beholder—the witness—and then doubly refracted in the eyes of the jurors as triers of the facts. This is the essence of our unique unscientific limitation. But recently law has reached for scientific tools to aid in its search for truth. Fingerprinting proofs, heredity blood tests, lie detectors; handwriting and typewriter experts, serve as a few examples. These work mainly in testing of the facts. The application of science to the creation of the rules is far more novel and of course meets with far greater resistance from the leaders of the bar, a resistance no more insidious than can be described by the state-

ment that so-called successful lawyers—the leaders of the bar—are those who have been richly retained by the people in society who have the greatest wealth, which in turn means those who are least likely to favor new rules in a game where they won under the old rules.

As a corollary it might be said that law as such determines to a great extent the potentials of scientific advances in any period. This we do by tools of various types—tax laws, through impacts of deductibility of contributions; the grants of monopoly to inventors or, rather, to the stockholders employing the inventive minds of the nation; rules of evidence to include or exclude expert testimony—these are a few examples of why we should become increasingly concerned with breaking through the self-contained mysticism of the law.

I have referred briefly to the prejudices I hold so that my further remarks may be properly discounted. Let me refer to one further, profoundly religious attitude I hold as an article of faith scientifically unprovable anywhere in the history of man, and directly pertinent to the few observations I shall make about the Kinsey report. We in this nation have taken one fundamental gamble: we have staked our all that in the free market place of thought, by the matching of ideas, truth has a better chance of winning than any other method known to man. This is the First Amendment to our Constitution. This is our Bill of Rights. There are some without such a dedication to the democratic search for truth; there are so-called leading scientists who condone the Iron Curtain and give comfort to dictatorship over the mind of man. For them my remarks may be irrelevant. I do suggest that freedom of scientific inquiry and free exchange at present worrying you and me are not unrelated to a profound faith in the right of all people to see, to hear, and to read.

Against this very brief profile of the law I now say that the Kinsey report is the single greatest contribution of science to the rule-making part of the law in my lifetime. And I do not overlook the impact of the so-called Brandeis Brief, the fact brief which contributed so much to a shift of the rule-making part of the law in such areas as, for example, hours of labor, minimum wages, and the like.

The Kinsey report broke through a mass of taboo. It must be evident that law cannot remain too far out of step with the desires of society; the social lag is at times due to the fact that means of communication, now concentrated in the great mass media, are themselves lagging in their responsibilities. Law, save for murder, divorce, and rape, is inadequately reported in the press and

radio, just as scientific advances are deemed by the lords of the press and radio to be uninteresting compared to the marriages of a movie star.

The Kinsey report smashed a taboo, a result scarcely anticipated by the Kinsey staff, and be-grudged by many of my friends in the anthropological, psychiatric, and medical fields. And remember that this first report is only a partial sampling. I can assure you that no bar association, law school journal, or lawyers' committee can proceed to consider laws or the judicial administration of laws dealing with sexual matters without reference to the Kinsey study. Kinsey's first volume ended an era. No longer will the background of hush and pretend prevail in this area of the law. Already the report has affected the two leading opinions written in the past year in censorship cases.

This has peculiar significance for those who believe in freedom of the press, the matrix of our laws. From 1787 until 1870 the restraints of the law on writings were based on the concept of blasphemy. From 1870 to 1915, during the Age of Comstock, manuscripts were subjected to the fear of test by such subjective unscientific words as "lewd," "lascivious," and "obscene." From 1915 to date, the book publishers have been fighting for their freedom. It has been my joy to defend the printed word on many occasions. In 1924 men went to jail for selling Joyce's *Ulysses*; in 1934 it was legally distributed. *An American Tragedy* was legal in New York and not in Massachusetts. New Mexico had no censorship laws when Massachusetts had its book-burnings in the late 1920s. Are fewer or poorer contraceptives used in Connecticut and Massachusetts, where birth control is illegal, than in all our other states where contraceptives are lawfully dispensed? What is the effect on sexual behavior patterns? Might not laws be affected by facts? Only half a dozen states precensor movies—I may say unconstitutionally, but with the blessing of Eric Johnston and the movie industry. Our screen diet is imposed against the unscientific concept of the word "obscene"—a concept which must be read against the sexual habits of our people. Some might urge that vague, unproved, and to date unprovable standards defining obscenity have some value as a kind of societal placebo. I submit that, whatever may be argued for medical placebos, no society should consciously condone a cultural practice of cultured placebos—since deception as a technique is a force for infantilism and surely negated by the gamble of our Bill of Rights. For a free people no government we elect should be permitted to hand out sugar pills or offer a govern-

mental kiss to cure a social evil or deflect social thought from anything but a search for truth.

The Kinsey report, even this one first volume, has been of mighty help in this area of the law. By hunch, and hunch alone, I have never wanted to go before a jury in an obscenity case. Kinsey validates the hunch. He gives us our first clues to explain why a magistrate suppressed *The Well of Loneliness* while later, on appeal, it was approved for distribution; why in another field a police court judge will levy heavy sentences on homosexuals, and an upper-court judge will call me to the bench and say, "Can't we get this poor devil a psychiatrist?" It is no answer for some of my friends to tell me that they always knew this or that fact which Kinsey reports in detail. Knowledge up to now was no more valuable than a secret, anonymous, unpublished autobiography.

Let me mention a few items on which law must say thank you. Our laws have attempted to abolish all sexual outlets except marital intercourse, nocturnal emissions, and to some extent solitary masturbation. The first Kinsey report says that 85 per cent of all younger males interviewed are criminals, since they make use of other sexual outlets. Remember the difference between criminal and convict relates only to the fact of being caught. Since such laws are of necessity capriciously applied, including spite as a means of caprice, it is unimportant if the Kinsey figures are not accurate to the last final decimal which his staff may find to be the over-all national, all-religion, all-race pattern of behavior. Law is so short of any scientific data on these subjects that it must of necessity use a big paintbrush, truly unconcerned at this time with final slide-rule accuracy. Eventually precision will be more important. For example, is there more fornication in Louisiana, where it is not a crime, than in Arkansas where a first offense is appraised as worth \$20.00 to the state and a third offense at up to three years in jail? What about the amount of sodomy in Georgia, where the punishment is life, compared to New Hampshire, where it is not covered by a special statute? And if you have seduction in your heart, or wherever it resides, I suggest you pick Vermont or Utah rather than Georgia. It may spell a difference of twenty years of freedom to you.

Don't let your amusement at these state-by-state mores lead you to disdain the great contribution of the United States by its adherence to a Federal structure, for this results in forty-eight separate competing laboratories called states. Imagine what would have happened if we had tried during the taboo era to establish a national pattern for sexual

behavior. I suggest we would have reached the lowest common denominator, as a national law on divorce would no doubt be also recorded today, conforming to the notions of the strictest state.

Forty-four states have laws against adultery. There have been only a handful of prosecutions. Yet the Kinsey report may well in its final national over-all figures show that one third of all husbands should be in jail if fact and law were the same. And this is only under one law.

The institution of marriage is everywhere being criticized. The Kinsey figures on admitted adultery may well be the first scientific guide toward revision of our laws concerning it. Under New York's divorce law, for example, adultery is the sole criterion of the state's interest in the happiness of children of an unfortunate marriage. The Kinsey figures will not long permit adultery to be the sole ground for the state's releasing children from a home continued by unhappy, destructive parents. When the next volume comes out, the patterns for wives will start to be unfolded, and, above all, we shall read comparative data.

Those who are concerned with juvenile delinquency, treatment of homosexuals, and the frightening attitudes of our penal institutions will have a glimmer of what the Kinsey report will do to the stream of law. Its facts raise questions that cannot be put to sleep or censored into silence. With enough scientifically gathered facts, shall we not be able to approach laws touching on sexual drives with something more than shame or feigned self-righteousness; with more of an eye toward cure than reprisal; and, above all, will not parents have further curiosity and hence insight into the dark crevices of guilt and the dangerous peaks of sheer adventure? It is no secret that the leading penologists have called in the Kinsey staff to reappraise penal patterns in one state institution. Might not the law, with sufficient factual material, be able in time to distinguish between the treatment objective of an embezzler and a Peeping Tom? Are the three concepts of punishment—a deterrent to others, a cure of the apprehended, and protection of society—equally applicable to those convicted of sexual crimes and those apprehended for other antisocial acts? I assume that even my Freudian friends will grant the possibility of some distinction along these lines at this stage of man's lack of knowledge about man himself.

In brief, I know of no field of law so deeply bedded in the bed itself, overlaid by attitudes of shame which fend off the scientific detached eye of science, as the laws of marriage, the relation between the sexes, and the sexual attitudes within the

sexes. Law has represented our concealed conflict with the stern puritanism which for economic survival had to deny the congruity of sexual behavior and joy; the historic patterns of development of man through fear and guilt; and the present religious movements which in theory, at least, try to make concomitant the assumed sexual relation within marriage for joy with the duties of procreation.

All our sexual laws have been swathed in cultural fantasies. To become adult, law must react toward a more adult society; that is, an informed and, I hope in time, critical society. There are those, even among scientists, who have been heard to say that such writings as Kinsey's should not be made public, that the Kinsey report is only for scientists and lawyers and ministers. I reject such attitudes, for I remember that the censors who read for obscenity ten hours a day always say that it won't hurt them but they are afraid for you and me. Such conceit coming from scientists is in itself a self-defeating approach to life and to the fuller

development of man. At all stages of man's history there have been those who said: "This is fit only for *our* eyes and ears. The people are not ready for it." Such attitudes we of the first freedom reject. And of all knowledge needed by our jurisprudence, that dealing with man's own behavior is the most important. No man can get outside his own frame of reference, and without knowledge cannot even operate decently for himself, his family, and society *within* his own frame of reference. All people are entitled to all possible knowledge held in the treasury of man dealing with man. The knowledge of a society reflects itself into the culture of the law. In so far as law bespeaks or bends folkways touching on sexual drives, it requires such research as the Kinsey report, a vitally needed document, for it deals with the area of our greatest taboo.

Whenever we deal with areas of man's darkness everyone seems to have the answers. I do not so pretend but suggest that here, as so often in life, the questions are often more important than the answers, and the law needs the questions which the Kinsey facts compel.

An Anthropologist Views the Kinsey Report

RALPH LINTON

Dr. Linton (Ph.D., Harvard, 1925), whose special fields of interest are oceanic ethnology and personality and culture, has been Sterling professor of anthropology at Yale University since 1946. Dr. Linton's paper was read in his absence by his colleague Professor George P. Murdock, of Yale. Dr. Murdock's Comments follow.

I UNDERSTAND that I have been requested to review the Kinsey report in terms of the anthropological viewpoint rather than in those of my own personal reactions. Since anthropologists are highly individualistic even in their views of what constitutes the proper field of study for their science, I approach such an assignment with considerable hesitation. However, I believe that I can say with certainty that the scientists of this discipline were neither shocked nor pained by the conditions which the report revealed. All the practices discovered by Kinsey in our own society are regarded as normal in one or another of the hundreds of so-called primitive societies which are the anthropologists' chosen field of investigation. (I will revert to this fact later when I come to consider the validity of certain of the authors' views on the instinctual basis for various forms of human sex behavior.)

Anthropologists also found nothing extraordinary in the marked divergence between popular beliefs regarding American sex behavior and the actual behavior revealed by the report. All societies show some degree of difference between what anthropologists call the ideal and real patterns of behavior within their culture. The ideal patterns represent the society's concepts of how people should behave in various situations. No society has such patterns for all forms of cultural activity, but every society develops them in connection with those activities which it considers important and to which it attaches emotional values. The ideal patterns are conscious and verbalized and, as such, are transmitted from generation to generation on very much the same basis as proverbs or riddles. They come to constitute the proper verbal responses to particular situations, but only the exceedingly naïve take them at their

face value. The rare attempts of individuals in any society to live according to its ideal patterns are foredoomed to failure. The normal members of all societies learn to transmit such ideal patterns at the verbal level while modeling their actual behavior on what they find that other members of their society really do.

In this connection, Kinsey's emphasis on the different patterns of sex behavior in different classes is of particular interest. One wishes that he had given more information on the size of the samples on which his conclusions regarding various classes were based. It is also to be regretted that in a country as varied in its population as the United States no attention seems to have been paid to ethnic, regional, or community differences. Although it might have been difficult to report on some communities without threat to the complete anonymity of all informants, ethnic and regional differences could certainly have been included. One suspects that reticence or otherwise in regard to sex matters and awe or otherwise toward the magic name of science both vary considerably from one social group to another, so that in some groups the individuals who were willing to be interviewed would be atypical. Both the educated and the criminal groups might be cooperative, but one wonders what proportion of small businessmen or skilled craftsmen were willing to be interviewed.

In spite of the authors' emphasis on class distinctions, they fail to make the basis for such distinctions clear. The one factor to which they constantly revert is the educational one, and their statistics show significant differences between males who have finished grade school, high school, and college. However, if this criterion is enough in itself to distinguish class levels in American society, this situation certainly deserves special investigation. It would seem to indicate a much closer correlation between economic levels and cultural differentiation than has been revealed by any other studies. The high correlation is the more surprising since the differentiations based upon school attendance would cut across a whole series of first- and second-generation immigrant groups whose parents and grandparents must have arrived in America with markedly different systems of sex mores. If correct, it would suggest that the child learns most of his sex attitudes and behavior patterns from his schoolmates of the same economic level rather than from his home environment.

Still another problem arises in connection with the class differences stressed by the authors. Al-

though few social scientists accept the American equalitarian myth in its popular form, the fact remains that there has been a higher degree of social mobility in the United States than in any other modern society. Kinsey points out that individuals rarely change their sex behavior after their middle teens and that lower-class men who have achieved wealth and social distinction tend to continue their lower-class patterns of sex behavior even in their new social environment (p. 437). Most upper-class Americans, especially if we use economic criteria for determining membership in this group, are removed by at most two to four generations from ancestors who were laborers or artisans and small farmers. How have these startling changes in both their ideal and real patterns of sex behavior been brought about?

In reading the report I could not avoid the feeling that the record was concerned mainly with groups at the top and bottom of the social scale. However, in emphasizing the differences between these groups in patterns of sex behavior the authors have performed a valuable service. The average educated American is likely to take ideal patterns of the upper classes in our society more or less at their face value. He may be quite conscious of his personal deviations from these patterns, but he usually regards himself as aberrant in this respect. Both the greater reticence with regard to their own sex behavior exercised by most members of the group and their culturally indicated reactions toward individuals who violate the class mores must contribute to this belief. Moreover, the upper-class individual tends to project these ideal patterns upon lower-class individuals. The moral superiority of the poor but honest over the wealthy and of the simple farmer over the city slicker are long-established traditions even among the better-educated.

The ideal patterns of the upper class are already familiar to most readers of the report, and the authors can discuss the degree of deviation from them shown by the actual data without troubling to state the patterns themselves. It would have been extremely valuable, however, to put on record what are the ideal patterns of sex behavior for middle- and lower-class groups. They mention the intolerance of lower-class individuals toward masturbation and their relative tolerance of pre-marital intercourse, but it would be interesting to know how far these attitudes are recognized and verbalized. Although the inquiry reveals many facts about behavior, it reveals comparatively few opinions about behavior. Since the psychological connotations of acts depend largely on whether the

individual thinks what he is doing is normal or abnormal, right or sinful, it seems that an important area has been overlooked.

Most of the foregoing have been minor criticisms intended to suggest ways in which the report could have been amplified profitably. However, there are other features of the work which, I believe, arouse much more active opposition among anthropologists. No member of the profession with whom I have discussed this book has failed to take exception to the title. All of them feel that the work should have been called not *Sexual Behavior in the Human Male* but "Sexual Behavior in the White, English-speaking North American Male," or words to that effect. The work leans so heavily on biology that many of the cultural determinants of sex behavior have been ignored. For example, the total range of sex behaviors represented in this volume falls far short of the actual range of sex behaviors in our species. The anthropologist can show that various societies habitually employ positions in intercourse which the average American would regard as impossible to anyone but a professional contortionist. There are also forms of erotic excitation more bizarre than any practiced by the most liberated members of our upper classes or by the professional prostitute group. There are some societies in which all individuals accept homosexuality as the normal form of sex expression for persons in certain age groups; other societies in which overt homosexuality seems to be genuinely unknown. Wide as the area of behavior covered by the report is, it by no means covers the possibilities. It represents developments within a definite cultural frame and cannot be understood without constant reference to that fact.

Although I have found no statement by the authors as to what they regard as "normal" sex behavior, one gathers from numerous remarks in the analyses of the material that they feel that there is such a thing. As a biologist, Kinsey seems to be looking for some noncultural type of sex behavior which would manifest itself if various cultural limitations and conditionings could be eliminated. Moreover, he seems to feel that the lower an individual stands in the social scale the more closely his sex behavior is likely to approximate that of the hypothetical natural man. It seems to me that this idea is as unrealistic as the eighteenth-century concept of the purity and high motives of man living in a state of nature. No matter how low an individual may be in the social scale, he is still a member of society. He has been subjected from infancy to social conditionings, and his sex practices will be those indicated by the subculture of his par-

ticular social group. To the upper-class observer members of the lowest groups may appear to be uninhibited and free from taboos, but they also adhere to the culture patterns of their own group. In fact, it would seem, on the basis of the authors' own data, that conventionality, in the sense of closer adherence to the declared patterns of the subsociety and less tendency toward experimentation, increases rather than decreases as one descends in the social scale.

The search for noncultural "natural" patterns of sex behavior is, I believe, a vain one. The reason for this lies in the very evolutionary trends which have produced our species. In another article of mine, published before the Kinsey report appeared, I said that man is an anthropoid ape trying to live like a termite. Although human societies bear a superficial resemblance to those of certain insects, they involve beings with quite different potentialities. The insects have discovered that you can do it better with instincts, and many human social planners must wish that their subjects were similarly equipped.

The evolution of mammals has been characterized by an ever-increasing sacrifice of instincts to learning ability. By the time the primates emerged, instinctive behavior seems to have been reduced to a minimum, and the instincts that had survived were of the simplest sort. They were related to behavior necessary to the individual's survival in the earliest stages of his existence, before he had had time to learn. At the human level, it seems exceedingly doubtful whether there are any instincts which are operative after infancy. There may be tendencies toward certain forms of behavior in certain circumstances, but such tendencies are always shaped by learning. I should make it clear that I am using the term "instinct" to mean genetically determined patterns of behavior. This must be sharply distinguished from the various drives or tensions in the individual which furnish the incentives for behavior. Most if not all of these drives are shared by men and animals, but the ways in which men satisfy their drives are always learned. This learning may proceed by the laborious process of trial and error or by the intellectual processes which serve as a substitute for this in solving problems, but it is most likely to involve the imitation of other individuals. All human beings owe most of their behavior to instruction by, or imitation of, other members of the same society.

The sex drive must be assumed a priori to be one of those which is most flexible in its expressions. It cannot accomplish its biological purpose of species

perpetuation unless its expressions in two individuals are mutually adjusted. Moreover, in our own species, these two individuals have been undergoing separate series of conditionings for many years prior to their union. From this fact alone one would expect that the sex drive could be satisfied in many different ways, and the diversity of "normal" patterns of sex behavior in different cultures would seem to bear this out.

Everything indicates that the sex behavior of human males, with the possible exception of nocturnal emissions, is always learned behavior. It has no instinctual basis unless we take as such the physiological pressures which make the individual seek relief without indicating how he can get it. It follows that one might expect the greatest range of individual, as distinct from culturally determined, variation in those societies like our own where there are strong taboos on the direct transmission of sexual techniques from adults to pre- or early adolescents. Patterns of modesty, privacy, etc., would make this tendency even stronger. Such conditions might well lead to the development of personal behavior patterns through trial and error or as a result of chance contacts with atypical members of the society. Perhaps the greater range of sexual behaviors observed by Kinsey in the upper-class group might be explained by the smaller opportunities of young members of this group to obtain concrete knowledge of sex techniques.

It is safe to say that there is no such thing as normal human sex behavior aside from the standards of normalcy established by the individual's society. Sex behavior is the most complete and most frequently cited example of cultural relativity. In fact, all discussions of cultural relativity seem to come back to it within the first two or three paragraphs, passing lightly over the much more uniform attitudes of various societies toward such things as murder or theft. It should be noted, however, that all societies do have patterns of what they regard as normal sex behavior and do punish deviations from these norms. Even Kinsey, in spite of his scientific detachment, seems to draw the line at rape and the molestation of minors.

The only thing about sex behavior which is biologically prescribed is that some outlet for the sex drive must be provided. What that outlet is can be determined by the society within very wide limits. Our own upper-class sex mores include a series of ideal patterns which are actually unworkable at certain points. Thus the insistence that males abstain from all forms of sex satisfaction before marriage, when coupled with the economic limitations on age of marriage, will inevitably be honored more in the breach than in the observance. Absolute faithfulness after marriage is also difficult to enforce and will be as long as human beings are gifted with imagination. The attitudes of most societies on these two points are of interest here. A very large proportion of the world's societies permit premarital relations between adolescents, and there is some reason to believe that this is an aid to the individual in making adjustments to his later status of husband and family head. It is certainly a help in breaking the silver cord. On the other hand, practically all societies regard lasting unions and those in which the partners are faithful to each other as the most desirable unions. Although attitudes on homosexual behavior vary greatly, even those societies that permit it tend to limit it to the adolescent age group, or to regard such relations as definitely inferior to heterosexual unions.

All this would seem to indicate that there are some forms of sex behavior which have greater social value than others, whereas any form of sex behavior that does not involve deprivation or attempted suppression of the sex drive can be made satisfying to the individual. The report suggests that our legal regulations should be made more realistic, but it seems certain on the basis of cross-cultural evidence that we shall always have definite patterns of approved and disapproved sex behavior. Moreover, it seems possible to train the bulk of a society's members to adhere to the approved patterns without any feeling of deprivation and without invoking extensive legal sanctions. What we need is a franker appraisal of the situation and a clearer picture of what sorts of sex behavior are socially desirable.

Comments by George P. Murdock

I AGREE only in part with the criticisms voiced in Professor Linton's paper, and even those I accept I regard as much less serious.

In general, I prefer to estimate a work of science in terms of the contribution it makes to knowledge

in the particular field rather than contrast it with a perfectionistic ideal of all the other contributions it might have made. The Kinsey report has by no means satisfied all our scientific curiosity about sex behavior in the American male, but it

has done much more than add to our knowledge of the subject; it has multiplied it. The defects of the volume, real and fancied, have been paraded in numerous symposia. What concerns me is that this hubbub of criticism may distract attention from its genuine merit.

The statistical techniques can certainly be improved somewhat—and no doubt will be in future volumes. But let us not forget the incurable tendency of statisticians to concentrate on the refinement of their manipulative techniques to the exclusion oftentimes of the most obvious scientific inferences about their data. The pollsters who predicted the election of Dewey by an overwhelming majority used sampling and other statistical techniques of high refinement, and then assumed that the people who registered themselves as "undecided" would vote for Dewey like the rest. A minimal knowledge of psychology would have told them that people who are not strongly motivated regularly act in accordance with prior habit. Since American voting habits had become strongly Democratic under four successive Roosevelt administrations, the "undecided" should have been counted heavily for Truman. It was not the polls that deceived us, but the statisticians who interpreted them. In light of facts such as these, I expect far more illumination from the careful scientific scrutiny of Kinsey's data than from minor improvements in his statistical procedures.

In the first Kinsey volume the total American population was not adequately sampled. But no pretense to this effect was made. The sample will be achieved, according to plan, only when the

project is complete. In the meantime, criticism on this score seems unwarranted.

I sympathize with the psychiatrists who wish Kinsey had plumbed more deeply into personality factors. But I refuse to ask for perfection, and cannot convince myself that a zoologist is blame-worthy for not venturing so far from his own field of knowledge. Moreover, criticism on this score would come more gracefully from psychiatrists if the profession were not so notoriously negligent in testing the adequacy of its diagnoses and therapeutic techniques against the subsequent history of its patients.

Least valid of all, it seems to me, are the criticisms leveled against Kinsey's interviewing techniques. This is a subject on which anthropologists are peculiarly competent to speak, for we have developed the most exacting techniques for discovering evasion, falsehood, and exaggeration in our informants. From all I can learn of Kinsey's interviewing techniques and precautions, they inspire in me the highest degree of confidence.

One final word. The anthropology of sex, after Kinsey, will never be the same. Now that we know the extraordinary range of individual variation in male sex behavior in one society, our own, we can no longer assume that actual sex behavior in any society will conform even approximately with the culturally patterned ideal norms. In future field work we shall have to explore meticulously the incidence of actual behavior. This will ultimately lead to a body of comparative data on the sexual behavior of all mankind that will be appreciably more realistic and dependable because of what Professor Kinsey and his associates have wrought.

Impact of the Kinsey Report on Opinion and Attitude Research

HUGH J. PARRY

Dr. Parry, formerly with the Opinion Research Center, is now with the Anti-Defamation League, of New York City.

IN THE program listing for this symposium, I was listed as representing the field of statistics.

I would like to modify that listing. Perhaps what I do represent is the somewhat larger and vaguer field of opinion and attitude research—that half-mapped area where statistics and social psychology and sociology and anthropology and almost every discipline under the sun come together. The opinion researcher in a sense has no formal

discipline of his own. Instead he must construct his own field of operations, utilizing material from other, more securely established areas, often picking up knowledge as he goes along. Sometimes I suspect that our range of information is inversely proportional to its depth. Whatever the case may be, I feel it necessary to warn nonstatisticians that I am not a statistician. It will be not be necessary to inform actual statisticians of that fact.

In discussing the contributions of the Kinsey report, one is drawn inevitably and temporarily to the question whether the project, and in particular its publication and mass distribution, were good or bad. Over and above attacks of varying degrees of value on the validity of the study, we have heard the theme of advocacy of suppression and censorship. The argument generally goes something like this: Whether or not Kinsey's work is valid, the results should not have been publicized but instead confined to statisticians, medical men, social scientists, and the like. If the American people learn that their sexual behavior is in conflict with certain conventional beliefs, then they may be encouraged to inaugurate or to increase sexual activities. Indeed, they may even be tempted to modify the statutes and the mores until verbal sanctions and prohibitions are more in line with actual behavior.

Most of us are acquainted with some manifestation of this argument. It has been endemic in popular magazines, and in the public statements of various public figures who do not hesitate to rush in where scientists fear to tread. This reaction bears many of the stigmata of blind panic. I forbear to analyze it, for the job has already been done in lively fashion by Leo Crespi in his "Moral Implications of Prevalence in the Kinsey Report" (*Int. J. of Opinion and Attitude Res.*, 1949, Fall). However, I cannot refrain from citing to you a little gem of almost transcendental *Urdummheit*. It is the statement of an "anonymous" (and perhaps imaginary) college professor appearing in one of two symposia conducted by a well-known digest magazine which reaches some eight—or is it eighteen or even eighty million?—helpless readers every month. I quote: "I dare not allow my name to be published . . . but I could testify under oath that since the publication of one recent survey on the number of illegitimate pregnancies among the girl students has been multiplied four times over."

The late Jimmie Walker, Mayor of New York, once testified at a legislative hearing on censorship that he had known of young ladies who had been seduced, but that the seduction was not customarily performed by a book.

My own beliefs on publication are quite clear: I do not accept the right of censorship over social research by any individual or group—and particularly so when the justification is that of protecting the American people for their own good. With Jefferson, I have a shrewd suspicion that the great mass of people in the long run know what is good for them, when they are not prevented from get-

ting the facts. I am also inclined to question a verbal code of behavior which is persistently and widely violated in practice, apparently with no great ill effect. The primary contribution of the Kinsey report, then, cuts across all lines: it is the mapping-out, roughly or accurately, of the conflict between action and convention in a major area of our culture.

Before discussing the contributions of the Kinsey report to opinion and attitude research, let me say a word about validity. Some of the unusual methods employed by Dr. Kinsey and his associates, particularly in the areas of sampling and interviewing, have given rise to considerable, and often acrimonious, controversy about the validity of *Sexual Behavior in the Human Male*. On occasion, the framing of the controversy has implied a certain naïveté concerning the concept of validity. Validity is not part of a dichotomy or of an either-or situation. It is rather a scale or a continuum. At the one end is an idealized "perfect" validity—a perfection, I suspect, that not even our elder brothers, the physical scientists, reach. At the other end of the scale is something approaching an absolute zero of validity. In between these imaginary absolutes lies the field in which scientists, physical and social, must work.

Thus a piece of work is not valid or invalid, period. It rather occupies a certain position on the continuum. A lack of specific knowledge at present makes it impossible to place the Kinsey report, with finality, on some point of the continuum. I suspect that we must take some of it on faith. Certainly it is still possible for two persons to examine the data and to reach two different conclusions as to validity. Thus, it would be presumptuous on my part to state flatly that Kinsey's results are valid or not valid. I can only say that on the basis of certain revealed data and on the basis of certain imponderables, I would place the Kinsey report on the plus side of the validity continuum. We shall know more when Dr. Kinsey is able to publish a more detailed account of his procedures.

What has the Kinsey report contributed to my own field, opinion and attitude research? I propose to discuss the contributions under several broad headings, and then to mention contributions that might have been made—lost opportunities.

First of all, in the area of questionnaire construction. Apparently, Kinsey did not use a questionnaire, in the standard sense. His first volume merely presents the skeleton of a questionnaire, listing the maximum number of subjects covered. I am assuming that the interviewers were allowed considerable latitude in framing their queries de-

signed to elicit factual information. Standard operating procedure in opinion research demands a fixed set of questions where opinions are concerned, but allows some leeway in eliciting factual information of a nonemotional sort. But in between such clearly defined areas is the broad field of factual information which is strongly tied to social pressures: specifically, even so simple a thing as "Did you vote in the 1948 presidential election?" In general, such information is gathered by a single standardized question, or by the use of a standardized indirect method, but, in any event, the process is standardized. A little later I shall mention just how true the responses to such questions may be.

By using a rather informal questionnaire, Kinsey has allowed a great deal of option to his interviewers. Undoubtedly such leeway would be disastrous with the "average" interviewer, but given a situation with a very few highly trained interviewers, Kinsey's work buttresses the claims of the informal questionnaire. Tentatively, then, the Kinsey report indicates the value of the informal, "subject" questionnaire in areas where pressures for social conformity are high, and where it is advisable to allow especially trained interviewers to vary the approach with the respondent.

I do not think that the Kinsey report has contributed greatly to the sampling process. The methods used were admittedly of a *faute de mieux* variety. His process of taking adequate but disproportionate samples of subgroups and combining them by a weighting process is a well-known method. It definitely increases the margin of error, however. Obviously, where the contrasting behavior or attitudes of many subgroups are the desired objects of measurement, such a method applies if one is to avoid the unnecessary expense of a huge sample. Certainly one can pay tribute to the ingenuity with which Kinsey has attempted to solve his problem, but the contributions of the Kinsey report are other than those of sampling design.

Perhaps a word should be said about Kinsey's often-criticized use of "volunteers"—most of whom, it has been implied, are atypical or psychopathic. Well, to some degree all respondents in every survey are volunteers. The number of un-reached respondents in a probability sample, for example, may run as high as 30 per cent. And, as Dr. Kinsey has pointed out to me, most of his respondents were volunteers more in this sense than in the sense of being eager exhibitionists. They were not persons who rushed forward to testify, but persons who agreed to cooperate, often after long and persistent effort on the part of the

interviewers. Most of them represented 100 per cent or about 90 per cent membership of groups. In his first volume, Kinsey has emphasized that about a third of his respondents represented 100 per cent membership of groups. He has added that about nine tenths of the remaining respondents represented groups from which a 90 per cent membership or better was taken. Thus, we may have doubts about the typicality of the groups themselves, but we can hardly postulate that the groups were composed of almost nothing but peculiar exhibitionists.

Before mentioning the contributions of the Kinsey report to interviewing techniques, I should like to say a word about the validity of respondents' answers. Some of you may have heard of an important address delivered before the American Statistical Association. The speaker, Don Cahalan, referred to a series of validity experiments conducted in Denver last spring and summer. It was found that untrue responses to certain "prestige" questions were given by as many as a third of the respondents, and that even in answer to nonprestige questions there was a certain proportion of untrue responses. The Denver experiment and most other studies of this nature have shown that the net distortion of truth in responses to prestige questions is invariably in the direction of social approval. Thus a man will say he has contributed to the Community Chest, when actually he has not; he will say he has voted, when actually he is not even registered; some years ago, he might deny that he had cashed in his war bonds, when actually he had done so. We might postulate that the amount of nonvalid response is directly proportional to the intensity of the social pressures involved in the subject of questioning.

I think we can fairly assume that the average person would minimize the amount and variety of his sexual experience, or avoid answering the question rather than exaggerating or inventing. The very intensity of the howls of protest from some quarters when Kinsey reported his findings bears testimony to the strength and direction of the social pressures.

The very fact that Kinsey undertook to investigate this subject should have an effect on opinion and attitude research. It is fair to say that in the past we have tended to avoid questions likely to arouse great emotion or conflict—so-called "personal" questions—simply because we have been aware of the great chances of distortion of response. We have usually either confined our investigations to relatively low-emotion items, or have approached the high-emotion items through scales

and other indirect means. The Kinsey report, in its interviewing techniques, has deliberately tried to counter the expected amount of nonvalid response—not through ingenious questionnaire construction, but through interviewer action. Each interviewer was also a planner of the project. Each interviewer was of a caliber far beyond the average. Each interviewer was allowed and encouraged to exercise his own judgment in obtaining information. One might say that Messrs. Kinsey, Pomeroy, and Martin were operating in an area somewhere between opinion and attitude research and psychoanalysis.

The interviewers, first of all, assumed sexual activity on the part of respondents and buttressed that assumption with the techniques of "rapid-fire questioning" for the purpose of making it hard for the respondent to rationalize or evade and of "forcing a subject" to make it still harder (Kinsey, *op. cit.*, pp. 54-56). As Kinsey himself pointed out, there was a certain resemblance to a police cross-examination. The validity of response was tested by such methods as comparing the answers of 231 pairs of spouses, and by a comparison of responses against known physical findings (*ibid.*, pp. 126, 130-31).

Obviously, a large amount of latitude given to interviewers may well create the danger of very large deviations caused mainly by the personalities and techniques of the respective interviewers. Comparison of the results of the three interviewers from comparable groups indicates that the latitude given did not create serious variations between interviewers (*ibid.*, p. 134). I should say that I am not thoroughly convinced of the adequacy of the ingenious checks used, but an involved analysis of Kinsey's validity is not the subject of this paper. I think it can be said that Kinsey and his associates have been able to counter the force of social pressure on respondents. Whether their techniques in turn have created a distortion in another direction is another matter. Their checks, admittedly partial and somewhat inconclusive, imply that they have not done so to any great extent.

It should be said rather strongly that in its interviewing techniques the Kinsey report violates most of the canons of opinion and attitude research. For years we have been trying to do something about one of the weakest links in the chain—the interviewer. We have tried to train him to follow instructions. We have tried to devise sampling techniques which will take all option of choice from his hands. We have bound him by a rigid schedule of question order and wording so that he will not attempt little variations of his own.

We have ordered him not to throw in gratuitous comments or to explain questions or to press the respondent. We have even ordered him to keep his tone of voice and expression as neutral as possible lest it affect the respondent. In short, we have tried to make of the interviewer an impersonal machine; and there are obvious good reasons behind this line of attack. How far we have succeeded is another matter. No one dare speak positively on the subject until publication of the important Social Science Research Council-National Opinion Research Center Interviewer-Effect study. It is safe to say, however, that we have not had striking success in solving the interviewer problem.

It is possible that we never will come very close to solving it by making our interviewers standardized. If this is so, it may be time to consider whether the whole approach is wrong. Most of us, whether we attempt to predict or not, have been unduly affected by the somewhat fortuitous connection between opinion and attitude research and election predictions. In the latter, obviously a high degree of precision is needed. In other types of opinion research, a greater margin of error is allowable. The robot interviewer is valuable where a high degree of precision on some rather simple dichotomous subject is needed; but perhaps it is the wrong approach to more complex areas.

I am merely offering this hypothesis for consideration. I simply do not know the answer. If we reverse the present trend in interviewers, however, and decide in favor of a relatively small corps of highly trained specialists, each allowed wide option, I suspect that one of the places we shall go for information is the Kinsey report.

A word about interpretation. Dr. Kinsey insists that he has left interpretation to others. I am inclined to agree with Dr. Clyde V. Kiser that social scientists should draw conclusions and "present the broad social implications of their materials" (A Statistician Looks at the Kinsey Report, reprinted from *Problems of Sexual Behavior*, p. 2). Interpretations will be made inevitably, and it is well that they be made by those most competent to interpret—the original investigators. But to some degree, Dr. Kinsey is making his denial in a Pickwickian sense. There are plenty of conclusions drawn implicitly, and some of them are social criticisms of the highest order. One is occasionally reminded of Samuel Butler's famous chapters on "The Musical Banks" or "The Rights of Vegetables." I think in particular of Kinsey's paradox of the policeman, and of his poker-faced calculation of the proportion of American males deserving of prison sentences. These, too, are con-

tributions, if only to the pleasure of the reader.

In discussing the contributions of so staggeringly large a piece of work, it may almost seem unfair to speak of contributions that might have been made. Yet, for my field of interest, the might-have-beens are even more important than the accomplishments. First of all, I wish Kinsey and his associates had not limited their operations to the field of behavior. I wish they had explored the attitudes of respondents, not only in regard to sexual matters but in many of the tension areas of our culture. Kinsey might answer that he was interested solely in behavior and in factual data. I can only reply that such an interest is not quite broad enough. We have now a beautiful set of data on behavior of factual subgroups, and this alone is valuable. Nevertheless, to some degree it represents a distorted picture.

Undoubtedly sexual behavior is a function of such things as education, socioeconomic status, and the like; but still more it is a function of certain psychological factors; it is a function of certain basic assumptions and attitudes. If Kinsey had added items designed to tap this area, we might by now have an even more valuable set of data. For example: Of those who engage in the most common illicit sexual practices, how many at the same time accept verbally a rigid code of sexual morality? Do they rationalize the conflict, and in what ways? Or do they admit the incompatibility of their actions with their beliefs?

I would like to have seen enough attitude questions so that we would be able to divide respondents into four major groups: those who upheld verbally and who also practiced what might be called "traditional" or "conventional" sexual mores; those who upheld convention verbally but acted otherwise in practice; those who advocated advanced sexual mores, but were conventional in practice; and, finally, those who both advocated and practiced advanced sexual mores. I suspect that the second group, those who merely paid lip service to convention, would be the largest. I suspect also that there would have been a large amount of guilt and insecurity found within this group. I would like to have seen the attitudes of the socially schizoid groups measured in other areas—in the field of intergroup tensions and prejudice, for example. We are rather sure that the highly prejudiced person is the aggressive person, and that behind this aggression we are apt to find guilt and insecurity and verbal acceptance of conventional stereotypes. I wish that Kinsey had been able to provide material to cast more light on this hypothesis.

Other questions come to mind: What are the correlations between what might be called "sexual conservatism" and conservatism in various other areas? To what extent do we find a fairly consistent liberal or conservative personality? How extensive are the areas of conflict between verbal acceptance and action in the sexual sphere? In what groups is this conflict most extensive? I am sure that any social scientist could go on from here and add several dozen other questions. But perhaps it is unfair to blame Dr. Kinsey for what he did not do. I suppose what I am saying is that I wish he had investigated the things that interest me. And yet I cannot help regretting some of the data that might have come from the project.

There is one other thing I wish had been done. Perhaps it can still be done. I would like to see Dr. Kinsey set up a probability sample of some 1,000 cases for a medium-sized city. All the respondents would be chosen on a name basis. Then I would like to see Dr. Kinsey's subject questionnaire used on this group. By checking various records and by indirect techniques we could get a great deal of factual information about each respondent, whether or not he or she was willing to participate in the survey. I would like to see the level of turn-downs and compare it to more normal surveys. To some degree a direct comparison could be made between those who were interviewed and those who refused or were unreachable. I would like to see the results of this project compared with results from one of Dr. Kinsey's comparable groups. In short, I would like to get specific data on the typicality of the Kinsey report respondents. Such data would give us an idea of what to expect from surveys in such emotionally loaded fields of investigation. What I am really asking for is a control group gathered by more customary sampling methods. Perhaps Dr. Kinsey has something like this in mind for the future. If not, I urge it on him very strongly.

Let me state again that the inauguration of the project, bold and imaginative in conception, is of the highest value to the social sciences. Perhaps its substantive findings, important though they be, are of lesser value than the pioneering aspects of the work. If I may quote myself in closing, "Perhaps Kinsey's most valuable accomplishment is that in an increasingly authoritarian age, with its conflicting religious and secular priesthoods, he reaffirms the rational pragmatic values of experimental science in a field of human existence hitherto given over to dogma and fiat" (Hugh J. Parry. Kinsey Revisited. *Int. J. of Opinion and Attitude Res.*, 1948, Summer).

The Kinsey Report and Society

MANFRED S. GUTTMACHER

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PSYCHIATRY, in the early days of its existence, when Kraepelin and his co-workers were bringing order out of clinical chaos, laid emphasis upon classification. Clinical research was on the superficial descriptive level. Large numbers of patients were examined objectively. Masses of statistics on familial neuropathic tainting, on minor anthropometric variations, and on neurological findings were reported. This important era helped lay the groundwork for modern dynamic psychiatry, but in itself this mass statistical approach proved to be sterile. It failed to explain why the majority of individual patients become sick or what made those who recovered get well.

Bleuler, Freud, and Adolf Meyer then began their advocacy of the intensive study of the individual patient. This approach breathed vitality into psychiatry, and men for the first time really explored the fascinating recesses of the sick mind. The psychiatrist suddenly acquired great intellectual satisfaction from his work. As a result of this evolution, there has developed, among many psychiatrists, skepticism and even aversion, for statistical studies and tabulations. Doubtless the pendulum has swung too far. For instance, too little attention is being paid to the important statistical studies of a variety of psychiatric conditions being carried out by the comparative study of a large series of monozygotic and dizygotic twins by Professor Kallman, of Columbia.

The large number of statements and reviews issued by psychiatrists on Kinsey's *Sexual Behavior in the Human Male* immediately marked it as one of the most important and provocative works on human behavior that have ever appeared. There is a remarkable variation in both the content and tone of these reviews. The legal member of this panel has made reference to the "rather arrogant language" in which these pronouncements were couched.

There are few disciplines in which divergent views are held to more tenaciously, or defended with more heat, than in psychiatry. When we see such reactions in our patients, we readily interpret them as evidence of insecurity. When they occur in our colleagues, particularly in those colleagues who are in agreement with us, we are tempted to look

upon them as utterances of a spirit that has been liberated from the neurotic necessity of too nicely maintaining the social amenities. Psychiatry is in truth a youthful science; moreover, as a discipline, it feels insecure, not only because of its age but because, unlike most sciences, it works very largely with imponderables that are incapable of quantitative measurement or exact proof. Consequently, I fear that we may seem given to establishing our positions with undue vigor.

The smoke of battle has in the two years intervening since the publication of the Kinsey report lifted so that we can now make a rapid survey of the chief points of attack on Kinsey's work by his leading psychiatric critics. Several have asserted that there are passages in the report that are unduly critical of psychiatry and psychiatrists. The strong personal supporter of Professor Kinsey, Albert Deutsch, although not a psychiatrist, holds this view.

Criticisms have been directed at Kinsey's handling of certain psychiatric concepts. Although they do not touch the essential value of his work, these criticisms are not without a certain validity and importance. There has been dissatisfaction with the treatment of the concepts of pregenital sexuality and sublimation. Informed psychiatrists do not hold the view that neuroses develop from the patient's social conflicts over his deviant behavior, as Kinsey reports. It is rather held that the deviant behavior is itself a symptom of the underlying neurosis. Those psychiatric concepts which are included in the report are often oversimplified and somewhat naïve—for example, "individuals become paranoid in their fear of transgression," and homosexuals can be converted to heterosexuality "through friendships which lead to affection." And, from my own rather extensive experience with senile sex offenders, I feel that the report takes too benign a view of their activities—few of them restrict their actions to affectionate, parental-like fondling. But these seem to me to be details that are not of fundamental importance.

Of much greater moment is the fear, expressed by many, that direct questioning is not a method that elicits accurate information in regard to sexual behavior, even by Kinsey's skillful technique.

Dr. Robert Knight, one of the ablest psychoanalysts in the country, wrote:

A patient's consciously honest replies to questions may significantly distort the facts of his life experience, because of several factors.

1. Memory recall, especially of ages when certain events happened and of frequencies of certain events, is subject to emotional moods for both exaggeration and minimizing.

2. Denial of certain past events through repressions is well known, and conscious honesty and cooperative answering cannot alter such forgetting.

3. Confusion in retrospect of fact and fantasy is quite likely to cause many subjects to declare that certain events really happened, when actually they were only vivid fantasies or even daydreams. All of us retain "screen memories" which serve to conceal or deny actual events, and these screen memories have a high degree of reality "feel" to us.

4. With few exceptions, events are never recorded in memory in terms of figures.

5. Events are remembered by association with other events in the context, but after a considerable lapse of time the contextual associations which could temporarily locate a given event, are likely to be lost or distorted by retrospective alterations.

Dr. Knight concludes, "Unless one assumes, then, that these disturbances of memory cancel each other so that the statistical averages of data from a number of persons, each of whom is unwittingly falsifying his own past experience, are accurate after all, then all figures obtained by such oral, single interview methods must be viewed with doubt."

He suggests that some psychiatrists familiar with free association techniques study intensively a number of patients and then validate Kinsey's findings against their own, to determine to what extent they are accurate. The chief flaw that I find in such a proposal is that the free association method has itself never been fully validated for accuracy.

Dr. Lawrence Kubie, one of the most brilliant and vocal of the psychoanalysts, attacks Kinsey for his concept of normality that, because something is widespread in human behavior, it must therefore be regarded as normal. He points out that in times of epidemic the common cold may infect more than 50 per cent of a population and that this does not make colds "normal." David Levy comes to Kinsey's defense. He says "a clinical norm is a frequency value. It is not a measure of health or morals. It is an index of commonality, or of characteristics, physical or mental, which people in a given culture, at a given time have in common." I fear that this is the kind of semantic quagmire in which we psychiatrists sometimes find ourselves. I am certain that many, along with me, have used "normal" to express both concepts.

Jules Eisenbud, among others, seriously questions the theory that the frequency of orgasm is "primarily a reflection of innate biological capacity." The authors' somewhat novel point of view, that precocious ejaculation indicates superior organic endowment, is in conflict with the observations of many clinicians.

The Kinsey report has been criticized from many quarters because it so largely neglects the highly complex psychological components of sexual behavior. It is felt that a more accurate title for the report would be "The Sexual Orgasm in the Human Male." The authors themselves anticipated this criticism, as they did so many of the others. They stated in their book that their study was to a large degree a study of overt sexual experience.

In regard to this problem, Professor Kluckhohn, the anthropologist, writes, "Kinsey and his associates show full awareness that the study of sex is not exhausted by a consideration of physiological and overt sexual behavior. Yet one may justifiably regret the lack of interest in the psychological involvement or consequences of sexual feeling. . . . *Sexual Behavior in the Human Male* is taxonomic in the limited as well as in the admirable sense."

I recognize that this lack of basic psychological data is of serious significance. Undoubtedly, it limits the usefulness of the study and at times makes for ambiguity in the interpretation of data. However, I do not see how the authors could have handled the psychological aspects much more fully without having laid themselves open to serious criticism. There are those who have expressed doubt in regard to the accuracy of the data on overt sexual behavior obtained by the authors' question-answer method, because of the distortions unconsciously created by defense mechanisms. It is apparent that this technique would be of more doubtful validity in eliciting accurate statistics on such complex phenomena as sexual attitudes and emotional responses. There the unconscious distortions would be far greater. Perhaps sometime in the future the authors will collaborate with competent psychologists and psychiatrists in planning such explorations. It seems to me altogether sound for them to find out what is happening before they attempt to understand why it has happened.

Although the authors have maintained that their investigations have been primarily involved in eliciting observable data on overt behavior, it is clear that they have a deep interest in the underlying psychological phenomena. And they have arrived at some very important conclusions in regard to them. These are best summarized in the paper that they read last year at the thirty-eighth

annual meeting of the American Psychopathological Association. I quote:

In brief, the psychosexual pattern in the human animal originates in indiscriminate sexual responses which, as a product of conditioning and social pressures, become increasingly restricted in the direction of traditional interpretations of what is normal or abnormal in sexual behavior.

This, it is to be noted, is not the Freudian picture of the development of the psychosexual through narcissistic and homosexual stages which lead, in maturity, to heterosexuality. Nor does it support that interpretation of masturbation or homosexuality in an adult as the product of fixation at immature stages of development. Our preliminary examination of over 7,000 histories of males, and nearly as many histories of females, shows that an exceedingly small portion of them ever passed through the hypothesized stages. Some children begin with an exclusively narcissistic interest in their own sexual responses, others begin with exclusively heterosexual activities, still others with exclusively homosexual play; but the great majority begin by responding to any sufficient stimulus. This is exactly what we would expect on the basis of all that we now know about the anatomy and physiology of the sexual mechanism.

Our interpretations seem to satisfy the facts more simply than theories of pan-sexuality, or of polymorphic-pervasive stages in psycho-sexual development. These latter concepts seem to imply that there are several different sorts of something called sexuality, and that all of these more or less mystic impulses are housed somewhere in the body of a child. On the contrary, we suggest that sexuality, in its basic biologic origins, is a capacity to respond to any sufficient stimulus. It is simply a picture of physiologic response and psychologic conditioning in terms that are known to the biologist and psychologist. This is the picture of sexual response in the child and in most other younger mammals. For a few uninhibited adults, sex continues to remain sex, however they have it. But for most persons, a repetition of experience inevitably develops a preference for particular types of relationship. When, as in our own society, there are tremendous rewards involved, and social reactions which may determine the lifetime happiness or unhappiness of an individual, it is inevitable that certain types of sexual activity should predominate and become, in the thinking of the majority of persons, the natural, the normal, and the right kind of relationship.

This is essentially a simple theory and easily intelligible. The fact that it will be vigorously controverted by many psychiatrists, particularly the psychoanalysts, does not diminish its importance. This theory fails to explain many cases of deviant sexual behavior with which I have worked.

In the space remaining, I should like to leave aside theoretical discussion and explain what the Kinsey report has meant to me in my work as a practicing psychiatrist and as the neutral psychiatric adviser to the courts of one of our big metropolitan cities. I began reading the report the day that it became generally available. It had a profound impact; immediately it impressed me as one

of the really important works of our times. I do not, in my clinical work, study a few cases with great intensity. I see large numbers of cases from all economic, educational, and racial groups. Many of the individuals that I examine for the courts are not sick at all, unless one holds the view that all criminals are *ipso facto* sick. On the first reading, before one begins to weigh and analyze a piece of research, one gets a certain "feel" for it. Mine was that here was a bold, vast project, brilliantly conceived, patiently and sensitively executed, and carried out with the greatest honesty. Even minor clinical points that I had run across in my experience were to be found here and gave, for me, an impression of authority to the whole work—cases of young adolescents who ejaculate under severe nonsexual tension stimuli, such as school examinations; the tendency of adolescent boys to tell you that they had stopped masturbating just a couple of weeks before you examined them; the suddenness of adolescence in boys compared to girls, etc.

I have been fortunate in having had the opportunity to meet and work with Dr. Kinsey since the publication of his study. The regard that I had for the research has been increased by my meeting with the man. I am *not* one of the workers in the field of human behavior who says nonchalantly, "There is nothing really new in the Kinsey report, we knew all that before." Why some of these wise-aces had not let some of us less well-favored students in on their knowledge is not clear to me.

The Kinsey report has already had definite effects on practical issues. I should like to cite two, with which I have been personally acquainted, as illustrations of their variety. When the report appeared, my eldest son was attending an old and well-known Quaker coeducational preparatory school, which was very strictly run. A group of the senior boys, who had been eager to liberalize the rules governing the fraternization of the sexes, marched on the dean, armed only with the Kinsey report, and miraculously won their objective.

While I was chairman of the Forensic Committee of the Group for the Advancement of Psychiatry, we issued a report on "The Psychiatrically Deviated Sexual Offender." We were fortunate enough to have Dr. Kinsey sit with us for two days as a consultant. Our report, which has been well received by many legislators, soberly warns against blindly going with the tide, which at times threatens to become a tidal wave, and passing harsh preventive legislation, which is likely to lead to abuse rather than cure. Kinsey's findings were the points by which we steered.

The debt that society will owe to Kinsey and his

co-workers, for their researches on sexual behavior, will be immeasurable. Nowhere do we so greatly need the establishment of scientifically obtained facts, to overcome blind emotional biases, as in the area of sexual behavior. Last December the Associated Press carried this dispatch:

Horrified by ever-increasing sex crimes against children, Representative Chelf (D., Ky.) today announced he will try to make it a Federal offense to molest a child sexually. Chelf would turn the FBI loose on offenders and set the penalties in a stiff range of ten years to death. Chelf, a member of the House Judiciary Committee, said he believed such a statute would be as effective in curbing degenerates who prey on children as the Lindbergh anti-kidnap law has been in controlling kidnapping.

The Congressman said it may be necessary to amend the Constitution before such a law can be enacted, and that he would propose such an amendment if that is needed.

He wants penalties of ten to twenty years in cases where an attack-child survives, and penalties of death or life imprisonment, as directed by the jury, if the child is slain. He would bar the defense of insanity to persons accused of attacks on children, but allow mental treatment of those convicted and sent to prison.

Chelf, in a statement, said he has been shocked to read horrible accounts of the ever-increasing brutal sex slayings of this nation's children.

First of all, there are no statistics to support the statement that sex slayings of children are ever-increasing. Then, although severe penalties diminish the incidence of kidnapping, which is nearly always economically motivated, it does not even suggest that it would have the same effect on crimes that have a psychopathological basis.

We must have facts to fight such hysteria. Our lawmakers must be given some understanding of the various aspects of sexual psychopathology. This type of indiscriminately harsh punitive legislation was used widely in the eighteenth century, only to be given up in the face of a rising crime

rate. It pays us to keep in mind the fact that public hangings on Tyburn Hill—many of them of pickpockets—are said to have been given up because the London police were unable to cope with the pickpockets working among the crowd.

In my advisory psychiatric reports to the court I have quoted liberally from the Kinsey report. Judges are impressed by the printed word. Such a source book proves to be invaluable.

In closing, I should like to list the findings in the Kinsey report that have impressed me most. This is, of course, purely a subjective list, dependent on my own prejudices and experiences, and not representative of general psychiatric opinion.

First, the wide variations in all phases of sexual function and behavior that occur with great frequency in a normal population. The variations are much greater than in physiological functions that are not so intimately associated with cortical cerebral function—for example, basal metabolic rate, the chemistry of the blood, etc.

Second, that those who mature earliest sexually maintain potency longest.

Third, no statistical support was found to establish the existence of the male climacteric.

Fourth, the direct correlation of sexual behavior with the socioeducational status—not only present but ultimate.

Fifth, the high incidence of homosexual activity, and the relatively wide prevalence of animal contact among intellectually normal males.

Sixth, the high incidence of sexual practices in the United States that are in conflict with unrealistic legal enactments.

Psychiatry is eagerly awaiting the completed series of studies by Dr. Kinsey and his co-workers. They will surely leave an indelible imprint upon the psychiatric thought of our time.



Ecology and History

JAMES C. MALIN

Since 1921 Dr. Malin (Ph.D., University of Kansas, 1921) has been at the University of Kansas, where he is professor of history. His article is based upon a paper presented in a symposium on "The Orientation of Ecology," arranged by Dr. Charles C. Adams for the College Park meeting of the Ecological Society of America, September 1948. The article by George F. Carter in the February 1950 issue of THE SCIENTIFIC MONTHLY, "Ecology—Geography—Ethnobotany," was based upon a paper presented in the same symposium.

THE present discussion of the relations of ecology and history is organized around five points: A statement of premises incident to collaboration of the several disciplines across the boundary lines; some implications of a general ecological interpretation of history based upon variations in options made available by cultural changes; an ecological re-examination of the history of the United States, with emphasis upon methods; a review of the grassland problem as a concrete example to illustrate the meaning of adaptation; and, finally, a consideration of the grassland in relation to mechanically powered-minerals culture which provided the background for its occupation after about 1850.

The premises upon which the discussion rests include the dependence of society upon nature and natural forces, not upon a conquest of nature; the unity of knowledge; and the existence of a body of methodology sufficiently common to all disciplines to afford a working basis for collaboration in matters of interdisciplinary nature. Both history and ecology may be defined as the study of organisms in all their relations, living together, the differences between plant, animal, and human ecology or history being primarily a matter of emphasis. Therefore, all forms of single- or limited-factor interpretations are rejected as fragmentation of knowledge, with its resultant distortion of facts.

At any level, cultures afford man the opportunity to exercise options in ordering his relations with environment with himself, and as the culture changes the range of the options shifts. Since ecology has become a recognized discipline, it is appropriate to re-examine history with special reference to ecological relations and the significance of these shifting options; the Mediterranean-centered culture yielded to Western Europe, and then

to an Atlantic-centered pivot of world power. Included in this selective competition is the transit of culture to the world outside Europe, and then, since about the middle of the nineteenth century the invasion of the grassland of North America and elsewhere, together with the changing options under the influence of a mechanized-power culture. This article focuses attention primarily upon the meaning of ecological adaptation as applied to the United States.

The Turner Frontier Hypothesis and its Closed-Space Corollary

The validity of the Frederick Jackson Turner frontier interpretation of history has been challenged, and upon sound grounds within the traditional methods of writing history. But before leaving the problem of the frontier hypothesis, whether American or new world discovery and expansion of Europe, attention is called to another body of associated ideas, which may be challenged effectively by an ecological methodology applied to history. The term "geographical discovery" is, after all, a subjective term, purely relative in character, whereas scientific method presumes a definition of terminology in a form that is both objective and operational. The idea of discovery applied only to Europeans, not to the aborigines of the lands "discovered." The same is true of the concept of the "frontier"—the frontier of what? Obviously, the frontier of the culture of modern Europe. Again, what is the "new land" about which Turner and Halford J. Mackinder and others wrote so insistently? To whom was it new? Certainly not to the American Indian, to the aborigines of Australia, or of Africa, nor to the Chinese. Europe and European-peopled America

considered their culture a superior, or master, culture, and themselves perhaps a master race, and thought of "discovery" and of "frontier of new land" in a subjective and egocentric sense. For the most part, the invading culture refused to recognize that the displaced cultures possessed any values, or that the peoples concerned possessed any rights which should be respected. Newness, however, implies the operation of the factor of time, subjective and relative, and time is a ruthless leveler of persons and things. Given a sufficient lapse of time, the tables might be turned upon these invaders by another invading culture.

A scientifically conceived ecological methodology applied to human history would emphasize ecological competition of two or more cultures for dominance in given earth areas, which could be made as objective and exacting as when applied to plants and animals—competition of cultures of differing degrees of complexity, the ecologically stronger invading the earth area occupied by another. But such a methodology would avoid value judgments and would recognize instead differences and similarities. The land involved would not be new land, but land that has been exploited for unknown generations, and that, in consequence of cultural invasion, became subject to a mode of exploitation different from that under a previous culture, and in that sense the cultural techniques might be called new to the land of the displaced culture. The land was not new; only the introduced technology of exploitation was new to the land. In further development of this line of thought, newness is involved because the more complex invading culture possessed technological tools and skills which made available different or wider ranges of options as applied to the exploitation of the area, bringing into the flow of utilization existent resources that were latent under the displaced culture. That point deserves special emphasis. The earth possessed all known, and yet to be known, resources, but they were available as natural resources only to a culture that was technically capable of utilizing them. There can be no such thing as the exhaustion of the natural resources of any area of the earth unless positive proof can be adduced that no possible technological "discovery" can ever bring to the horizon of utilization any remaining property of the area. An attempt to prove such an exhaustion is meaningless, because there is no possibility of implementing such a test. Historical experience points to an indeterminate release to man of such "new resources" as he becomes technologically capable of their utilization. At one stroke, such a concept renders the Turner-

Mackinder doctrine of "closed space" meaningless, and correspondingly destroys the basis of the argument of the "closed space corollary to the Turner frontier hypothesis," which holds that a welfare state—a regimented social order—must be instituted to serve as a substitute for the "closed frontier" in order to preserve American democracy and opportunity.

Ecological Re-examination of the Data of History: The Occupation of the United States by Europeans

There are several other possible methods of approach to an ecological re-examination of the history of the United States. Four are indicated here. One fruitful method would be to trace the occupation of the area by Northern Europeans, allowing for a contrasting occupation of the Middle Americas by Southern Europeans. These Northern Europeans were the product of a forest culture in a maritime climate. For present purposes of stating the problem in terms of ecological data, the issues may be brought out effectively by asking a series of questions, but without now attempting answers. What ecological concepts or impedimenta did these Europeans bring to America as a part of their cultural heritage? What did they expect to find? What did they find that they expected, and what did they find otherwise than expected? How did they react to what they found? To what extent did they recognize differences? How did they make adjustments to the differences they recognized, that they did not recognize, or were slow to evaluate? How did that adjustment, or lack of it, or lag, affect their cultural concepts and behavior?

As the occupation of the interior of North America advanced, the population, accustomed to a forest-maritime environment, found it necessary to readjust to a continental climate and to a grassland. The series of questions formulated to apply to the transit of culture across the Atlantic applies equally well to the advance into the interior, but where the emphasis as between Europe and the American seaboard was upon similarities, the emphasis as between the maritime seaboard and the continental interior would be placed more sharply upon differences, especially west of the forest-prairie boundary. In both areas there would be substantial emphasis upon differences, however, as between the southern and northern latitudes, which involve temperature differences and short- and long-day photoperiodism.

A second possible approach to this ecological re-examination would be the history of the explora-

tion of North America by naturalists: the geologist, the botanist, and the zoologist. The story of the geographical exploration of the continent has been told frequently, but scientific exploration came relatively late, and the story has not been told adequately, if at all, for any field of science. Such historical writing as has been done in connection with science emphasized the most obvious aspect: collection of material, its classification and naming—the making of a classified catalogue. An ecological examination of this process would supplement and extend the record of what has thus far been primarily taxonomic.

A variant of the foregoing approach would be found in a series of biographical studies of leading naturalists who participated in the scientific cataloguing of North America. This would be done with a view to reconstructing their ecological outlook and reactions to the work upon which they were engaged.

The construction of ecological traverses of America, or of regions, is another fruitful procedure. The idea of the traverse is borrowed from the geographer, but the materials, procedure, and objectives would be ecological. The primary focus of ecological traverses would be historical. They would be run for several successive time periods and in several directions, especially east and west and north and south, through areas of different topography, altitudes, and rainfall patterns. The materials would be written documents descriptive of ecological facts which would be evaluated systematically. Such ecological traverses of chosen historical time periods could then be compared with current ecological traverses of identical routes based upon field studies of ecological survivals. I have presented sample studies for traverses, as of about 1849, in my book *The Grassland of North America*. These were undertaken experimentally, and the details of techniques and methodology need to be perfected as experience is gained in practice.

How much has man modified the ecological setting of history in America? No certain scientific answer can be given because the necessary historical-scientific investigations of an ecological character have not been made. The samples of ecological history completed indicate less fundamental change than is usually assumed by conservation propagandists. At present, answers must, perforce, be tentative, and largely a matter of personal opinion. This is peculiarly unfortunate when public attention is being bombarded by propaganda to authorize gigantic programs dealing with natural resources.

The Grassland Problem: The Meaning of Adaptation

When forest man met the prairie in the Ohio Valley and Great Lakes regions as the American frontier of white settlement moved westward, he was puzzled about the fact that these earth areas were covered with grass. A voluminous literature grew up dealing with the question of why the prairies were treeless. Although there may be exceptions, forest man tended to avoid settlement in the open prairies. If he took prairie land, it was contiguous to forest land, which he made the base of his farm establishment. Several reasons have been assigned for the retarded settlement of the prairies, some of which are inconsistent and apply obviously to different kinds of grassland: wetness, difficulty in plowing grass sod, low degree of fertility, prevalence of disease, lack of water, of wood, of navigation, and of protection against the hazards of climate. Because the problem has not been adequately defined, there is much contradiction, misunderstanding, and some controversy. Sometimes trees and sometimes grass occupied soil inferior for the standard agricultural crops of the area. It is possible, however, to trace the major steps in the substantial reversal of forest man's concepts about the soils of the grassland, but soil scientists generally have not yet arrived at an adequate understanding of them. A long list of other problems needs examination and reorientation in order to clarify history as well as to afford perspective on the present, both the successful adjustments and the numerous ecological blunders, and to define the meaning of adaptation.

The British breeds of beef cattle were introduced into the plains. One of these breeds, the Hereford, possessed latent characteristics that were released so conspicuously by the new environment as to enable it to dominate the region from Mexico to Canada. The beef and dairy breeds of Brahman cattle from Asiatic India possessed desirable characteristics for the hot, dry, Southern plains. Among field crops, the competitive experimentation with wheat varieties proved that the hard spring and winter wheats from Russia possessed adaptive capacities; likewise, the sorghums from Africa and Asia, mostly imported by way of the higher-rainfall East, established themselves in favor in various parts of the grassland; and, in addition, alfalfa from the Mediterranean area, by way of Chile, was introduced into the economy of California and the plains. It is important to emphasize that all the animals and plants involved in these so-called adaptations to the plains were introduced

without biological change from their original environments to the new, and that in the original environments they possessed all the qualities that they demonstrated under the new conditions. In their original environment, they possessed in some, and possibly in most, cases characteristics that were relatively unnoticed or latent, but which became conspicuous or even decisive under the options of the plains environment. The process was one of selective experiment with materials already in existence elsewhere, and no one understood the biological mechanism involved. Much is still not understood. Only in the second quarter of the twentieth century did the second, or creative, phase of biological adjustment emerge in effective form, based upon the genetics of mutation and hybridization and the correlation of breeding and agronomic programs with the principles of developmental physiology. This phase has only begun, its potential is unknown, but for the first time the ecologist and the historian gain a new insight into the meaning of adaptation, the mechanism of biological behavior of the past, and one which opens the door

to future adaptive breeding programs with chosen objectives.

Soil physics, tillage methods, and the tools with which to operate a farm were the product of the forest-maritime environment. Here again concepts stood in the way, as well as the practices and the tools designed to meet traditional conditions. But there was much in this cultural accumulation, latent or relatively so, that possessed values that became significant and important in the grassland: in plowing technology, the steel plow, the lister, and disc machinery; methods of handling the soil to conserve moisture and to retard wind erosion; harvesting machinery, particularly the header; and the beginnings of horse-powered agriculture.

This approach to history makes no commitments about the validity of the concepts of civilization, about its rise and fall, and about progress. What are they, anyway, but philosophical speculations? What is envisioned here is an intermediate, selective, ecological competition of people occupying earth areas under the changing range of options afforded by cultures of differing degrees of complexity.



THIRST UNANSWERED

There is so much to learn; where should I tell
The avid eye to focus, or the mind
To go adventuring, the heart to dwell . . .
In mysteries of Maya; the designed
And valid beauty mathematicians find;
The tsetse fly, or spectroscope of Mars,
Or whorls of economics, intertwined
With rhymeless rhythms of sunspots and wars,
Or chromosomes that rule inexorably as czars . . .?

There is so much to learn—small recompense
For thirst unanswered as the days drip by.
We loftily commend the simple sense
By which the bookless seem to live and die,
But all the glory roads that seek the sky,
Passed through the lone heart's unrefuted core,
Are bent toward Truth, as lenses qualify
The random ray; no knowing heart ignore,
If wisdom be your aim, and Truth your emperor.

AUGUSTA TOWNER REID

Alleged Obstacles to Social Science

GEORGE A. LUNDBERG

Dr. Lundberg, who is head of the Department of Sociology at the University of Washington, has studied at four American universities and taught in eight. He was the first director of the Bureau of Social Research of the Pittsburgh Federation of Social Agencies and is a former editor of Sociometry.

A GREAT deal of discussion has appeared in recent decades regarding the gap that seems to separate the physical and the social sciences. On the whole, the effect of that discussion has been to reduce the apparent exclusiveness of the subject matter of the two areas, at least so far as the application of scientific methods is concerned. Thus, Lancelot Law Whyte, a physicist, has recently suggested that "the coming period may see the establishment of a *single unified science* covering the inorganic and organic realms and also providing the valid scientific approach to the subject matter of psychology, and possibly also of sociology." This does not mean, as Whyte notes, that the biological and social sciences will be explained in terms of physics, but rather that the phenomena of physics and sociology will appear as special cases of principles so comprehensive as to include all these special principles now constituting particular sciences. K. E. Boulding, an economist, in "Is Economics Necessary" (*Sci. Mon.*, 1949, April), has recently advanced a similar viewpoint with respect to the division that at present characterizes the social sciences. He suggests, for example, that perhaps the economist's value theory is also applicable to other data than those with which economists at present concern themselves. That is, as soon as we develop units for measuring moral, aesthetic, affectional, recreational, health (etc.) values, all the considerable theory and technology that economists have developed in their studies of supply, demand, and prices will be applicable also to these other aspects of human social behavior.

Dr. Boulding has also contributed greatly by emphasizing the frequently overlooked fact that it is the function of pure science to construct systems of propositions relating quantities of *any* variables, and he refers in this connection to the theories of economists regarding "economic man." Such sys-

tems are, as he says, "models" describing how economic phenomena would behave under various "ideal" conditions, *ceteris paribus*, etc. Physical science has profited greatly from the construction of such theoretical models, working with ideal gases, perfect levers, frictionless motion, and many other such "hypothetical" situations.

I

There is great need for emphasis on this point of view, for there has been and still is much naïve agitation on the part of some economists and other social scientists against any kind of formulation in the social sciences which does not correspond with what critics choose to call "reality," by which they mean concrete occurrences of events in uncontrolled nature. Thus Donald L. Kemmerer, in "Are the Social Scientists Backward" (*Bull. Amer. Ass. Univ. Prof.*, 1948, Autumn), alleged that one difference between the laws of economics and of physics is that the formulations of the economist do not always (!) correspond to the actual economic events of recent or remote history, whereas, as he says, "the law of gravity never fails." It would be more nearly true, perhaps, to say that the law of gravity *always* fails because objects never fall according to the law, outside of the laboratory.

Another economist, S. Garbuny (*The Social Scientist of Today. Ibid.*, 1948, Winter), identifies the same obstacle in these words: "As soon as the assumptions are made by simplifying real conditions or ignoring existing factors, we no longer speak in terms of the real world. An object is created and studied that does not actually exist (p. 714). . . . Stability just cannot be brought into a subject by scientific method if the object of scrutiny itself is changeable" (p. 715). It is true that the latter author makes the reservation that "working concepts such as perfect competition,

equilibrium, or socialist economics, useful as they are, are equally far from actual situations. The point is, rather, that quantitative analysis must be supplemented in various ways." Of course quantitative analysis must be "supplemented." But why the implication that this is a unique problem in the social sciences?

Arnold Rose, a sociologist, brought forward the same fallacy when he advanced the thought that "social science does not attain universal laws but rather culturally-limited generalizations" (*Amer. Sociol. Rev.*, 1945, 10, 560). He overlooks the fact that most of the generalizations of physics are even more rigorously "culturally limited," namely, by conditions of a rigorously restricted laboratory "culture." Even more astonishing are the conclusions which this author draws from his fallacious assumptions about the intrinsic difference in the "universality" of physical and sociological generalizations. "As long as social science," he says, "deals with social phenomena on the level at which it [sic] can be observed directly, it cannot find cause and effect laws that apply universally in any culture, and that fit into a systematic theory of society" (p. 560). In a later paper (*Amer. J. Sociol.* 1948, 54, 219-27) the author recognizes the error in his assumptions regarding the "universality" of physical laws, but he continues to insist that propositions of the form "if a, then b" cannot be made in the social sciences. The fact, of course, is that in neither the social *nor* the physical sciences can such statements be made without (1) a rigorous statement of conditions and (2) a statement of the *probability* of the occurrence of an event under the stated conditions. With such reservations, statements of the type "if a, then b" are as possible in the social as in the physical sciences.

In short, the reasoning in economics about such imaginary concepts as "economic man" and his behavior is as permissible and as valuable as similar reasoning about ideal gases, perfect levers, etc. The justification in each case is the light which these formulations throw on the intolerably complex relationships of both physical and social phenomena as they exist in unclassified and uncontrolled nature. The simpler "models" serve as bench marks with reference to which the influence of multitudes of factors present in actual uncontrolled situations can be measured, as, for example, in the allowance we make for altitude, etc., in estimating how particular bodies will actually fall at particular places and times. Both physical and social science must organize their systematized knowledge in terms of such standardized models,

and must then for practical purposes measure the differences between these models and actual occurrences in nature, in order to modify the laws governing model situations so as to permit application of these laws in actual "natural" cases.

In spite of his lucid exposition of the above points, Boulding nevertheless falls into some other familiar errors frequently characterizing discussion of the alleged differences between the physical and social sciences. Thus, on the very first page of his paper he repeats the familiar cliché that "the social scientist faces a problem which normally does not bother the nonsocial scientist, in that he himself is part of the field of his investigation." This idea was expressed almost verbatim also by Julian Huxley some years ago in an address before the AAAS. The idea occurs with disturbing regularity in the pronouncements of both physical and social scientists. Consider the following recent statement by Garbuny:

The social scientist must realize that he is basically a part of the problem he is studying—humanity. He will soon learn that his object is not in a "rest position" as the physical world, but actually in a real tumult, constantly changing and driven forward by the decisions and actions of his fellow beings. For this reason, findings in the social sciences comparable to the measurements in the natural sciences are, from the very beginning, only of conditional validity (*op. cit.*).

The last statement betrays the real source of the difficulty, namely, the common assumption among social scientists that the measurements and statements of physical science are *not* of only "conditional validity." Obviously, *all* scientific statements whatsoever, in *any* field, are only of conditional validity. For all the teaching and talk about science, we seem to fail to get across to both physical and social scientists the central fact that the content of science consists exclusively of a body of related and verifiable statements of the "if . . ., then" type.

On examination it turns out, then, that statements like that quoted above are little more than figures of speech intended to call attention to the danger of biased observation and interpretation of data regarding which the observer may have strong feelings of one sort or another. But, obviously, the danger of biased observation exists in all fields and in all sciences. We protect ourselves against these human tendencies to error on the part of the observer by the use of instruments and through the discipline that constitutes scientific training. Granting that the social scientist, after a manner of speaking, is "part of the field of his investigation," is not the astronomer also part of the solar system? What, exactly, is the difference in this respect

between an astronomer photographing an eclipse and an anthropologist taking a movie of a religious ritual of a savage tribe? Or, if the idea that man is "a part of" the solar system seems far-fetched and oppressive to some, what about a physician taking his own temperature or a biologist analyzing his own blood? Here, surely, he is very literally "a part of" what he is studying. The whole problem is a superficial one arising from a failure to realize that the classifications of all natural phenomena are made by man to suit his sensory equipment in attempting to comprehend his universe, and everything is "part of" that universe. Verily, we observe in part and we prophesy in part; and we *classify* to suit our purposes in the scientific quest. The classifications are not imposed on us. Thus, for some purposes and from some points of view, the anthropologist may be part of the savage tribe he studies; for other purposes and from other points of view, he is not. In any case, it cannot be shown that man cannot study objectively something of which he is a part. So the alleged handicap of the social scientist in being "part of" what he studies is neither unique nor an insurmountable obstacle. Indeed, as we shall show, it may be an advantage.

Consider, in this connection, the following poser: "If the heavenly bodies were themselves moved by astronomers, or even if they were moved by temperamental angels who guided their behavior by the astronomers' predictions, astronomers would find themselves in just as bad a fix as the economists. The bacteriologist who must stain his bacteria in order to see them would be in even worse trouble if his bacteria blushed when they were observed" (Boulding, *op. cit.*). At first glance, this appears to be one of the most apparent and plausible differences between the social and the physical sciences. But is it a significant or fatal difference of the type alleged? Suppose the astronomers could move the heavenly bodies by derricks or magnets as scientists can manipulate the materials and the animals of their laboratories. Would it not increase rather than decrease their power of scientific study of these phenomena? Such power would make astronomy an experimental science like physics. As for bacteria (or girls) that blush when they are observed, three solutions long since invented by scientists seem applicable: (1) devise methods of observing under which the observed is not aware of it (e.g., one-way glass in nursery schools, etc.); (2) develop indifference to observation on the part of the observed (e.g., laboratory animals, including children, communities frequently surveyed, etc.); (3)

discover and measure exactly what difference in behavior results from being observed and allow for this difference in our correlations and conclusions (e.g., it has been discovered what difference altitude makes in the law of falling bodies, and we can therefore usefully employ the law at all altitudes).

In short, all laboratory experimentation is open to the objection above urged against the social sciences. The essence of experimentation is for the experimenter to possess the very powers that Boulding fears would raise havoc in astronomy. In justice to the author, it should be mentioned that he makes the following reservation: "Of course not even the astronomer seems to be exempt from observer trouble in these days of relativity" (p. 235). But it is not necessary to take advantage of these more recondite considerations in order to make the point at issue. Do we not intentionally "influence" the behavior of the white rat when we set up an experiment to study its behavior? Both the design of the experiment and the fact that we observe the rat's behavior are bound to affect that behavior. It might be argued, after the manner of the economist who objects to the study of "artificial" situations, that we are interested in the behavior of rats in their natural habitats and that the study of them under highly artificial laboratory conditions, therefore, is either unimportant or misleading. No one ever raises this point because it is well known that we learn very important things about rats (and other animals), including knowledge of vast importance in understanding rat behavior in its natural habitat, from the study of rats in laboratories. How can this be? Our studies enable us to arrive at *principles—abstract generalizations of general validity* that are applicable to large varieties of special cases. The whole manner of speaking about the observer being part of what he observes is obviously ambiguous and imprecise. In logic or in any rigorous manner of speaking, things are "part of" whatever categories we set up and whatever classifications we choose to make. Scientific laws apply, in any case, only to stipulated conditions, and it is for the scientist to select and to stipulate these conditions.

There remains what to many appears to be the crucial case: What about heavenly bodies controlled by temperamental angels whose behavior is modified by the very fact of the astronomers' predictions? A presidential address to the social science section of the AAAS some years ago put the matter this way: "One can talk about nature without disturbing nature, but one cannot talk about society without changing in some measure the whole course of social development." Is it not a fact that the be-

havior of people may be affected by the very generalizations and predictions made by social scientists about that behavior? It certainly is a fact. But what of it?

To deal first with the angels, it may be noted that astronomy seems to have achieved a very considerable development during centuries when it was, in fact, believed that the heavenly bodies, as well as other physical phenomena, were in a high degree controlled by precisely such temperamental angels. This did not deter men from observing, recording, and generalizing about certain regularities in the "temperament" of the angels.* Later scientists dispensed with the postulate that the regularities and the irregularities of natural phenomena were attributable to the whims of angels, but they continued to observe with increasing care and accuracy the regularities and the irregularities.

Since man is the only animal that employs to any considerable extent language and symbols, it is true that language, in the form of scientific predictions and otherwise, becomes a part of the environment which influences human behavior. But man's speech is merely one of the forms of his behavior which modify the world in which he lives. Engineering works influence the course of rivers, and vaccines influence the behavior of bacteria, as compared with their behavior before the scientist interfered with their "natural" courses or ways of life. As is well known, bacteria develop immunities or make adaptations to the new conditions imposed upon them by scientists. Does this destroy or make impossible a science of bacteriology? No, for the modified behavior itself becomes the object of further study by scientists.

II

Among the various apparent obstacles to the applicability of the methods of natural science to human social phenomena, one of the most general is the feeling that ethical considerations somehow will always in some degree make the basic methodological problems of the social sciences at least "different," if not insuperable. This conclusion has been eloquently stated by the economist we quoted at the outset, as follows:

For all the attempts of our positivists to dehumanize the sciences of man, a moral science it remains. . . . In a world of technicians, it is the economist who raises the cry that the technically most efficient is not necessarily, or even usually, the socially most efficient; that the best cow is not the one that gives the most milk; the best business is not the one that makes the most profits, etc.

* I have elsewhere pointed out that "free will" and "the will of God" are subject to scientific study and prediction in the same way. See *Can Science Save Us*, p. 93.

Much as one may share the author's tastes in these matters, it is nevertheless necessary to point out that his statements raise more questions than they answer. The question of what is the best cow (or the best steel, the best drug, etc., etc.) could have scientific meaning only after criteria of goodness in cows or in anything else have been rigorously specified. A scientist can then determine which of several specimens most nearly meets the specifications. But those criteria are not scientifically determined because, as we have emphasized above, scientific methods arrive *only* at "if . . . then" statements, never at unconditional statements of good or bad. If an economist declares that the best cow is not the one that gives the most milk, he must presumably specify what criteria of "bestness" in cows he has adopted. I challenge any economist and any scientist to show how, by methods recognizable as scientific, he will arrive at such a criterion. He may deduce from history what kind of cows have, on the whole and in the long run, contributed most to man's survival. He may take a poll of all peoples of all cultures that maintain cows as to what kind of cows they like best. He may collaborate with biologists as to what kind of cows are likely to live longest and give certain kinds of milk (etc., *ad infinitum*). But any or all of these criteria are *taken* by some person or persons belonging to some culture with moral, aesthetic, or other standards. These standards are not arrived at through any of the procedures recognizable as constituting the scientific method or methods. In short, a scientist's unconditional, aesthetic, or ethical decision that a particular cow or its milk is "best" has about the same scientific foundation as the cow's decision as to which is the best grass.

This is not to raise any question as to either the importance or the validity of the methods by which the chosen criteria are adopted. I merely point out the elementary fact that, when a man as a human being, strongly conditioned as a member of a sect or a larger culture to the moral, economic, or aesthetic standards of that culture, makes a pronouncement as to the "best" cow, the "best" tax system, the "best" medicine, etc., etc., he is necessarily only reporting on the degree of correspondence of a certain thing or desideratum to a standard—*any* standard—against which he elects to measure it. This standard of "bestness" may be *anything*, according to the purposes and desires of the person in question. Criteria of this sort are "taken," just as postulates are taken in the various systems of geometry. Man's purposes, desires, and preferences are, of course, influenced by all the factors in his environment that affect him at all,

and among these influences, science is one, but only one. To a person intent on suicide, the *best* drug is obviously the drug which, to a person intent on survival, is *worst*. To be sure, in this case a person (who may also be a scientist) from a culture in which suicide is regarded as immoral will, of course, declare the most destructive drug the *worst*. But unless he is very naïve he will realize that such a statement is not a scientific conclusion. How could it be, in view of the fact that, as I think scientists agree, *all* scientific conclusions are merely conditional statements of the type "if . . . , then"?

What seems to have confused the issue is the unquestionable fact that the scientist, by virtue of his skill in measurement, can certainly choose among various alternatives the one that most nearly fulfills any *stipulated* criteria, regardless of the scientist's personal tastes or preferences. That is, we may simply specify what characteristics a solution should have in order to make it most nearly adapted to purposes or results sought. The scientist can then certainly determine among various alternatives the one which most nearly fulfills the stipulated characteristics. An objective determination of this issue ("if you want this result, adopt alternative b") is obviously not to be confused with the processes by which we arrive at the characteristics desired. Thus, no one objects to Myrdal comparing the actual treatment of Negroes in the United States to the treatment stipulated in certain official and unofficial documents. What is objectionable is the further assumption that if we dislike the disparities revealed by the comparison, it has been proved scientifically that our treatment of the Negro is bad! The latter conclusion obviously depends on quite other than scientific criteria.

In short, each organism "decides" what is "best" according to what is called his taste, which basically amounts to saying that the organism decides the way it does because it is the kind of organism it is. This is also the basic reason why man at a certain stage accepts Einsteinian instead of Newtonian physics. Let us give full weight in this connection to what we know about the tremendous influence of culture, including science, in the determination of man's taste. That is still a very different statement from saying that our taste itself is arrived at by scientific methods, even if we postulate a wholly imaginary being whose tastes are wholly determined by the findings of science. Science or scientific method could operate as a determinant of choice only as a prediction of a consequence of a proposed behavior. The reaction (choice, taste) of the organism to the predicted consequence is not a scientific procedure. How could it be, if scientific

methods merely lead to "if . . . , then" conclusions?

My opponents on this issue seem to believe that scientific statements can be made which will hold without the conditional clause. This becomes a question of what is, in fact, the nature of scientific methods and conclusions. I am quite willing to leave that to any qualified jury of scientists and philosophers of science. I content myself with pointing out that even on the most advanced frontier of the most advanced sciences, the net *scientific* conclusion is never more than an "if . . . , then" proposition. If so, even the ultimate question of taste can be approached by scientific methods only in the form "if you want to survive, then do this and/or that." What I here call man's taste is, of course, merely the product of all the influences through evolutionary ages that have made any individual what he is at any given instant.

The confusion over this fundamental and elementary point seems to flow from an inability to distinguish between (a) moral statements made by moral men, who also happen to be scientists, and (b) scientific statements. The distinction is fundamental, and failure to observe it is, perhaps, the greatest obstacle in the world today to the recognition of the social sciences.

The reason for the reluctance to accept the viewpoint here advanced seems to be a certain disappointment that science with all its magnificent power and achievements cannot deliver unconditional, ethical imperatives, as well as perform the functions it is designed to perform, namely, predict consequences of any proposed behavior. Social scientists, especially, have emerged only recently and, for the most part, only partially, from a theological orientation. They still cherish the hope that scientific method will provide what the moral law written on man's heart by the finger of God is supposed to provide in the theological orientation, or at least in many people's interpretation of that tradition. That is, they want an infallible automatic mechanism which will protect man against himself—the hungers and conditionings developed throughout his whole evolutionary past. These scientifically overhopeful people desire not only a scheme by which man can lift himself by his bootstraps, but they envision a magic kind of bootstraps which themselves will tell man when and to what he should be lifted. The contrary view is that science is merely a very delicate, powerful bootstrap mechanism through which, under certain conditions, man may lift himself (if he pleases) at given costs, to certain limited and foreseeable estates. This is too modest and disappointing a program for many recent imbibers of the new wine.

III

Closely related to the aspect of science and ethics discussed above are the following:

1. It is pointed out, as scientists have always recognized, that (a) scientific method has its own ethics and that (b) every scientist indulges in evaluating (1) what problems to investigate and (2) what data are relevant to his problem. These considerations are taken to prove that scientific methods themselves call for evaluation, and therefore, it is implied, scientific conclusions are also ethical. To this reasoning there is a twofold answer: (1) Not all evaluation is ethical. There are no ethical questions involved in the statement that one object is heavier than another, although this is unquestionably an evaluation of two objects with respect to weight. (2) The scientific rules or mores against suppressing evidence and corrupting experiments are not themselves arrived at or tested by the scientific operations which resulted, for example, in the conclusion that one object is heavier than another. Likewise, a scientific conclusion that one tribe is technologically more highly developed than another carries with it no intrinsic ethical evaluation whatever regarding technological development. Scientific mores, like all others, are arrived at from experience and tested as to their value as means to desired ends. The end of science is a certain kind of understanding, but that end is not determined or imposed on us by scientific methods. It is *taken* because man's wants and interests are what they are. Man's wants and interests are what they are, as we have said, because of his whole evolutionary history in the world in which he has lived. All his characteristics, including his so-called inherent ones—habits, culture (including science), taste, wants, and goals—are the result of this life history.

It will be noted that we include science among the influences that have made man what he is. The role of social science among all the other factors influencing man's social behavior has thus far been small. It could become very important. But, as we have seen, scientific conclusions will always be in the form of a conditional statement: "If the spark [and all the other necessary and sufficient conditions], then the explosion." Such a statement can never carry any ethical implications regarding the social desirability of explosions.

Now the scientist, as a member of a community, is likely to have ethical views also on the latter subject. This has misled some writers into declaring that since the scientist is also a human being and a member of a community, with definite ideas about

the goodness or badness of various kinds of explosions, it is specious to separate his total behavior into (a) his role as scientist and (b) his role as citizen. Yet, clearly, we make such separations in all other lines of activity, including the professions. The physician is expected to treat patients he dislikes or whom he believes are a social menace with the same care that he treats his other patients. An actor wins applause for the excellence of his enactment of the villain's role, not because of his personal approval or disapproval of the character portrayed. The latter question is recognized as a separate issue of relevance in other contexts. Exactly the same is true of the scientist. He has performed his full function as a scientist when he has clearly depicted the consequences of a proposed type of behavior, as, for example, when he has accurately predicted an explosion. His applause or abhorrence of the explosion is not part of his scientific conclusion or function.

2. Another error regarding the relation of science and ethics lies in mistaking hypotheses, which are always present in scientific work, for ethical value judgments. Arnold Rose has said, for example, that "a statement of the type of solution sought is a value premise" (*op. cit., American J. Sociol.*). But the hypotheses of science are never statements of "conclusions sought" in the sense of conclusions *desired* by the scientist. A hypothesis disproved is a scientific contribution just as truly as one proved. The only kind of solution sought in science is confirmation or refutation of a hypothesis. It is probably true, unfortunately, that many social scientists today do regard their hypotheses as conclusions to be proved by appropriately selected data and reasoning rather than as questions to be impartially (scientifically) tested.

3. The persistence of the old dichotomy of "voluntary" and "involuntary" behavior further confuses the subject here under consideration. Traditionally, ethics is assumed to be concerned only with so-called voluntary behavior. The questions that have arisen in this connection are, in fact, the basis for the emergence of the value problem and ethics as a separate problem. Given a certain frame of reference, it can doubtless be logically held that only voluntary behavior is subject to ethical evaluation. This position has the further advantage that, by defining voluntary behavior in such a way as to exclude all behavior of the inanimate world and of nearly all the animals below man, the foundation is laid for the great dualistic systems of thought which find it necessary to deal with man in a different framework from the rest of nature. In the scientific orientation, voluntary behavior, to the ex-

tent that the category is used at all, becomes merely that behavior which is characterized by delayed response and which is mediated by symbolic (language) mechanisms. As such, voluntary behavior is subject to the same kind of systematic study as any other action.

That is, science circumvents the whole argument about free will by simply leaving this metaphysical issue alone and pointing out that the "free will" behavior, including the observed will of God himself, is just as subject to statistical study and prediction as any other kind of behavior. In the same way, science circumvents the problem which is perhaps chiefly responsible for the persistence of ethics as a separate field of thought, namely, the principle that man is morally responsible only for his voluntary behavior. This view is, of course, also enshrined in our criminal law, which rests squarely on medieval theology rather than on science. Science simply observes that nature holds man (and other animals) accountable for their involuntary as well as their voluntary behavior. That is, the poison man takes by mistake kills him just as certainly as that he takes deliberately. The "insane" (i.e., identifiable under present definitions of insanity) criminal is apprehended and incarcerated as is the "voluntary" criminal. It is true that radically different treatment is accorded the two, but both treatments have the primary purpose of protecting the community and rehabilitating (curing, reforming) the criminal. Except as a detail in the diagnosis and treatment of the case, therefore, the question of voluntary versus other behavior turns out to be irrelevant as far as the possibility of scientific study, social accountability, and free will is concerned.

The traditional viewpoint regarding voluntary behavior, however, still crops up as an interesting hangover in the discussion of the value problem by social scientists. First, there is the reluctant concession that "where human behavior is compulsive and unthinking, it probably lends itself to statements of invariable relationship. Such behavior would encompass infant development, mental pathology, crowd behavior, and perhaps other very limited areas of social study" (*Rose. Amer. Sociol. Rev.*, 1945, August, 560). The implication is that "thinking," "reasonable," and "normal" behavior is less subject to scientific study. Again, it is held that behavior that results from an organism's interpretations of stimuli is of quite a different order as far as scientific study is concerned as compared with behavior consisting of reactions "directly to

stimuli" (*Rose. Amer. J. Sociol.* 1948, November, 233), whatever that may mean. The fact is, I take it, that any response whatsoever is necessarily a reaction to the stimulus as interpreted and defined by the sensory equipment of the responding organism in whatever state of conditioning it may be. The elaborateness of the "interpretation" will, of course, vary according to the nature and development of the nervous system, including its conditionings and so-called apperceptive mass. That is to say, the interpretation and definition of the stimulus-situation to which an organism responds will vary according to the species, individual capacities, and the past responses, especially in the case of organisms capable of responding to symbols and their own symbolic constructs, memories, anticipations, predictions, etc. The "applicative mass" of traditional psychology is surely nothing more than the traces left in matter, organic or inorganic, by its previous behavior. That is, the tendency of a wire or a paper to bend more easily in a place where it has been previously bent is as much the "applicative mass" or particular "interpretation" of these respective substances as is the modification of human responses by education or conditioning of whatever sort. (See N. Rashevsky: Learning as a Property of Physical Systems. *J. Gen. Psychol.*, 1931, 5, 207-29.)

There appears, therefore, to be no ground whatever for injecting considerations of "interpretation," "definition of the situation," "applicative mass," "situation as a whole," etc., into a discussion of the applicability of the usual methods of natural science to human social behavior. These terms merely describe in greater detail the history and nature of the response and do not change the basic fact that, in any event, science observes and records the significant response regardless of the fact that some responses consist of simple reflexes, whereas others are highly mediated by conditioning, training, and symbolic mechanisms. Those differences are of interest from other points of view. But the present uses of these considerations in the social sciences constitute little more than a smoke screen of verbiage under cover of which it is hoped to reserve for social scientists certain privileges of a non-scientific character which are denied to other scientists. One of these privileges is that of injecting ethical judgments into generalizations about behavior and claiming that these judgments, as well as the generalizations, are arrived at by scientific methods.

Storm Clouds Over the Andes

C. LANGDON WHITE

The author (Ph.D., Clark, 1925), who has been professor of geography at Stanford University since 1943, has based his article upon a year's work in Peru, during which he traveled and studied widely throughout the sierra.

DOWN Peru's backbone (Fig. 1), at elevations of 9,000–12,000 feet (though often 14,000 or 15,000 feet), in a world apart, lives a large portion of the nation's total population. Millions of Amerindians, toiling on farms and in mines, are for the most part unreached by contemporary world events. These Indians are descendants of the once-mighty Inca civilization, but centuries of neglect and repression have left their mark on them, physically, mentally, and spiritually. The natives have been kept by their conquerors and by their natural environment in the mental mold of the sixteenth century. Today, after more than four hundred years of Spanish occupation and rule, nearly all the mountain Indians continue to use their own languages (unwritten)—Quechua and Aymara. As a result of this barrier of illiteracy, they have been prevented from taking a real part in the operation of their government. Basically they have been untouched by Spanish civilization.

In order to understand the way of life of this huge population in the sierra, it seems appropriate to present briefly certain high spots in the history of the aborigines of the pre-Columbian period. The past is thus presented less for itself than for the light it casts on the present, and the illumination it gives to the future.

Although Peru is one of the richest strikes archaeologists have ever come upon, no one really knows very much about the ancient civilizations of the Andes: speculation is resorted to at almost every turn. Even the authorities disagree passionately on innumerable issues. This results from the fact, first, that the Incas had no written language* and, second, that the Spaniards, in their conquest, destroyed fanatically, and killed the Inca leader Atahualpa and his close associates. This last deed had far-reaching repercussions, because the Inca was supreme, and with his murder the whole system collapsed. The masses, unused to making independent decisions, did not know what to do.

* Very lately a new alphabet has been developed, and work is beginning on book publication in Quechua.

A few things are known: About A.D. 1100 a superior people under the first Inca, Manco Capac, began to build in the cold altiplano of present-day southwestern Peru what was to become one of the most advanced civilizations and empires in the New World. As the Incas began to progress, they emigrated from their bleak home base down into the pleasant, mountain-protected valley of Cuzco.

Only in the light of the geographic environment can one appreciate the true magnitude of the task of empire-building faced by these people. Theirs was indeed a hard environment, consisting of endless tumbled ranges and mountains. The land was so high that even though it lay within the tropics

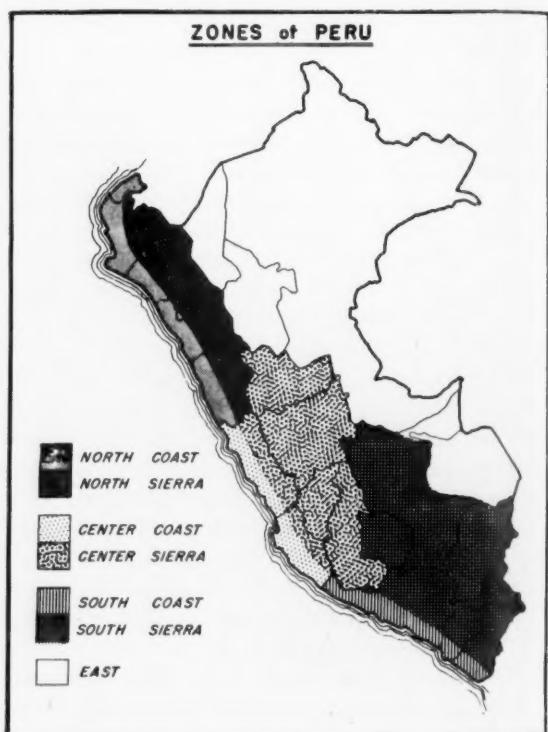


FIG. 1. The sierra, which comprises approximately 40 per cent of the total area of Peru, ranks second in size. It is the Indian region—most colorful and most interesting historically of Peru's three regions.

it had a severe climate. The cold was piercing and penetrating, and there was no fuel except llama dung, dried sod, and *yareta*—a mosslike plant. Notwithstanding, the Inca dynasty welded a host of tribes, having diverse languages and customs, into a political and cultural unit of about 10,000,000 people. Were this empire at its maximum extent to be superimposed along the Atlantic seaboard of the United States, it would extend from Maine to Florida and from the Blue Ridge to the Atlantic Ocean (Fig. 2). Theoretically such a region would appear fit only for producing and maintaining a backward, poverty-stricken people, but the Incas, by displaying exceptional abilities and through possessing an "in-born genius for growth and organization," fashioned their milieu into what became in many ways a model state. It has been accurately and succinctly said that everything in the highlands was inferior except man himself.

The empire was administered from the top by a dictator (the Inca), but the government was a benevolent autocracy. The people were in rigid strata, and no one could rise above the stratum in which he was born. All the land was the property of the Inca, and every man's labor belonged to the state, except for members of the royal family, priests, sons and grandsons of army officers, males under twenty-five and over fifty, the sick, and the incapable.

The Incas became truly great agriculturists. Their civilization, however, is believed to have been based not upon maize, as elsewhere in the high civilizations of the New World, but upon the potato. Much of the Inca homeland was too high for the successful cultivation of maize, but in areas less severe climatically, corn was grown. (Peru even ranks with Colombia and Central America as the possible original home of corn.) Not only are these Amerindians credited with having domesticated from seventy to eighty different species of agricultural plants, but, with only crude hoes and planting sticks as farm tools, they developed a scientific agriculture, which included the preparation of the soil, rotation of crops and fallowing, destruction of weeds, use of fertilizer (guano), the terracing of steep mountainsides, irrigation of dry land, and the development of special varieties and strains of plants through selective breeding. The making of these contributions took much of both intelligence and time.

This great agriculture was developed, moreover, without the aid of the ox, horse, or any other power than that of their own muscles; the only work animal the Incas ever domesticated—the llama—has never to this day permitted man to harness him to a



FIG. 2. The Inca Empire at its maximum extent.

plow or a cart (Fig. 3). Hence the footplow, a pointed stick with footrests and handles, which was guided by the hand, was employed. It penetrated only to a depth of about four inches. Women and boys knelt down in front of the "plowmen," turning over the sod as rapidly as it was pried loose. The footplow was well adapted to precipitous slopes and dwarf fields. Inca terraces are among the man-made wonders of the world. In the rugged sections, the restricted cultivable area was extended to the limit by terraces. Constructed of stone, they were so well built that even today, after centuries of neglect, many still are in use.

The Incas, however, were more than just farmers. They were skilled road builders and great workers in stone (Fig. 4). The southern sierra fairly bristles with distinguished art. (It is not definitely known that the Incas actually were the builders of Cuzco and the marvelous monuments in the Andes, or whether these were already established and were only conquered and improved by them. Many scholars believe that the ancestors of the Incas are probably responsible for the great megalithic ruins.) They were also great designers and dyers of textiles; in all probability they have never been equaled, and certainly never sur-



FIG. 3. Llama train in the Peruvian sierra. These animals are at home only in the loftier areas of Peru—upwards from 7,500 feet. This animal's operating cost is almost nil: he needs no costly pack saddle, he eats on the way, and he can carry about 100 pounds on his back, averaging 15–20 miles per day. To the Indian, the llama is more than a pack animal—he is a friend who shares the long journeys and the piercing cold of the bleak *puna*.

passed. The finest of their fabrics was made from the wool of the vicuña, softest of all animal fibers—270 threads to the inch as compared with 140 otherwise considered to be the world's finest.

During the reign of Huayna Capac, who was the last Inca to rule the entire empire, the domain had become so large that prior to his death he divided it into two parts, a northern and a southern, to be ruled by his two sons, Atahualpa and Huascar. Unfortunately, there was jealousy between the brothers, which resulted in civil strife and the defeat of Huascar. This occurred shortly before the arrival of the Spaniards. In 1531 Francisco Pizarro, with a band of less than 200 men and some 27 horses, descended on the Inca Empire. "It was not the valor of the conquistadors, nor their firearms, lances or swords, so much as it was their horses, that made it possible for a few Spanish soldiers to overrun so vast an area in so short a time, and it is certain that when the horse could not be used, the advance of the conquerors was noticeably slackened" (John J. Johnson. *The Spanish Horse in Peru before 1550*. In *Greater America: Essays in Honor of Herbert E. Bolton*. University of California Press, 1945). The story of the con-

quest is well known and has been graphically portrayed by many able historians. Suffice it to say that the Spaniards took advantage of the dissension between Atahualpa and Huascar, and overcame the Incas in November 1533. Theirs was a ruthless conquest, characterized by murder, rape, and terror. When they had finished, little was left but death and ruin. Cities were leveled, roads abandoned, terraces neglected, and the people enslaved.

Once the ruling group was destroyed, the rest was comparatively easy. The majority of the Indians, long accustomed to unquestioning obedience to central authority, accepted the new rulers. Thus the Spaniards took possession of both the land and its occupants. It was only where the Indians had been living in an environment where irrigation and cooperative living had been practiced for several centuries that the Spaniards were able to take over easily. Fastening themselves upon the Incan system, they—priest and cavalier—became the cornerstone of a new order. Thus the conquest meant at first mostly a change in rulers. How different the history of Peru (also of Ecuador and Bolivia) might have been had the sierra been empty or but sparsely inhabited!

During the colonial period the Indian population declined notably, the causes being epidemics brought by the Spaniards (the Indian had no immunity to smallpox, measles, and tuberculosis); forced labor in the mines and fields; destruction of the elite of the population with a political purpose of subjugation; and the unpardonable obligatory migration to zones of different climate.

Can anything at all be said for the conquistadors? They were indisputably vigorous and fearless men; they were energetic explorers and great conquerors. This was so because for centuries before Columbus reached the New World, war in the Old World had been the major occupation. The struggle to wrest Spain from the Moors inevitably produced such a breed of men. Thus in the Americas, nothing stopped them—neither heat, cold, precipitous slopes, jungles, swamps, nor warlike opponents. With them, however, came the Catholic clergy, who sincerely considered the Indian worth civilizing and his soul worth saving. In all probability Inca civilization might have been utterly obliterated had it not been for the Church.

The Spaniards also brought cattle, horses, asses, goats, and sheep, and they introduced such crops as barley, wheat, and sugar cane. They inaugurated many new farm practices and they built cities, universities, and monasteries. Moreover, they trained the Indians in European art and architecture, and an effort was made to blend the discordant Indian and Christian cultures into a harmonious balance in society, in art, in life. Although the mark was overshot, still the effort was not wholly without result. (Harold E. Wethey. *Colonial Architecture and Sculpture in Peru*. Harvard University Press, 1949).

What, then, of the Andean Indian today? The type of person he was prior to the conquest has been presented. What is he like now? The descendants of the highly civilized Incas are like most of their buildings, terraces, and roads—they are in ruins. Exploited for four centuries, they have become to a very great degree degraded peons. Only occasionally are they physically impressive. They wish only to be left alone. They resent inquisitiveness and protect themselves against it with apathy and indifference. As Kurt Severin expresses it, "The air is cold in the Andes and so is the heart and mood of the Quechua." The spirit that characterized their ancestors is gone. They deaden their misery by chewing coca and stimulate themselves by drinking *chicha*. Yet they are not completely immune to change. As the Peruvian Emilio Romero has pointed out, the Indians were



FIG. 4. Stone work at Cuzco. An example of the expert stone work of the Incas and pre-Incas in the southern sierra of Peru. All such work was done on a large scale and required great numbers of well-organized workers.

at first hostile to all progress; as they saw its benefits, however, they changed. He then cites how they strewed the newly built roads into the sierra with broken bottles, which ruined automobile tires. But as soon as they realized that they were not forbidden to ride on buses and trucks (as the Spaniards had forbidden their forebears to ride horseback), and that they could travel many miles for twenty centavos, large numbers were willing to work on roads without pay.

Yet the descendants of the once-mighty Incas are still not a part of the twentieth century; theirs is a medieval survival. Almost their whole mode of life is of the past—their food, their methods of farming, their agricultural implements, their homes, their dress, their means of transport (Fig. 5). Even their religion consists of a survival of that of their Inca ancestors, with a thin veneer of Christianity. Everywhere throughout the sierra the gods of the past are worshiped.

In a very real sense the mountain Indians are outside a money economy, for they buy little and sell little. Instead they barter in their famous Sunday markets much as their Inca ancestors did. Very little money is circulated. Thousands of Indians from miles about trudge to such a market as that held in Huancayo in central Peru (Fig. 6), and hundreds of items change hands in a few hours.

The Spaniards believed that conquered peoples should pay tribute. Thus there came into existence the system known as encomienda. Actually encomienda did not carry with it the right to the ownership of land, but the conquerors took possession of both the land and those who occupied it. Land would give the prestige they passionately wanted. In the more out-of-the-way corners, however, the Indian communities continued to use the land in the traditional way—the collectivist way—paying

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FIG. 5. Typical village scene in the sierra. Characteristically dressed, these, like most Indian women, are barefooted.

only a type of rent to the new owner. Consequently the two contrasting systems of land tenure have continued to exist together in the same general region.

Where the Indian still has land, he guards it with unbelievable affection. He fences it with a wall of stones (Fig. 7). He guards faithfully the boulders that mark its bounds. He suspects every stranger—white or mestizo. Though he can wring from it only the meagerest subsistence, the land, no matter how impoverished by its long use, is still his dearest possession. He leaves it only when he is forced to do so: "Centuries of occupation have fixed him to the soil." This does not mean, however, that he feels the need of individual ownership of land: at heart he is a collectivist.

The mountain Indians suffer from grinding poverty (Fig. 8). Their homes, food, and clothing all reflect this, and one sees human misery to a degree almost beyond belief. The houses are small, one-story, thick-walled adobe or stone huts, frequently of one room, with thatched roof. Often there is no window or door and, of course, no chimney. The family spends most of its time outside the house, for warmth is found there in the sun rather than inside. There was good reason for the important place of the sun in the Inca religion!

It is bitterly cold in the sierra. Hence wool clothing is worn for warmth. Sheep, introduced by the Spaniards, now supply most of the wool for clothing, though some llama wool is also employed.

Most Indian feet go bare, particularly among women and young girls. Walking over all kinds of terrain makes their feet as tough as elephant hide. Where sandals are worn, some are made from leather, but the majority are fashioned from old worn-out automobile tires. Men wear sandals, short black pants, shirts, vestlike jackets, and ponchos (Fig. 9). Ponchos are woven in one piece, with square corners and a hole in the middle for the head; they increase in length with increasing altitude and cold. On the head, under a felt hat, is a closely fitting woolen cap. Each adult carries his pouch of coca leaves. Women wear blouses, many, many skirts and petticoats of gay colors—orange, red, yellow, blue, and green—belts, felt hats, and bright, woven blankets slung over their backs. These blankets invariably contain produce, a baby, or perchance a live sheep. Fashion demands that natives of different localities shall wear hats of different patterns; hence, headgear varies greatly from region to region.

Seventy per cent of the total Peruvian population is reported to be illiterate. If this be true, cer-



FIG. 6. Sunday market in Huancayo, probably the most famous fair in all Peru. It is impossible to describe the great variety of wares, which range from corn, potatoes, and coca to live pigs and sheep, and to silver of exquisite design and workmanship. Through the centuries, along this street, which was a part of the imperial highway connecting the Inca capitals of Quito and Cuzco, passed rulers, explorers, conquerors, and liberators.

tainly at least 95 per cent of the mountain Indians must be, because those who speak an Indian language as the native tongue are considered illiterate. This results from the fact that so far Quechua and Aymara have not been made mediums for popular communication. The problem of education has many facets. To build schools and train teachers in so poverty-stricken a land is a slow and costly task. The caste system, moreover, prevents upper-class Peruvians from teaching in Indian schools. Thus education cannot reach the Indian from the top down. And if it is to come from the bottom up, how is it to get started? One of the brightest spots in this respect is the work of the missions of a North American sect; on the shores of Lake Titicaca it established a number of schools and trained a considerable number of teachers. The Indians have also requested schools of the government as provided for in the constitution. Consequently, more and more Indian children are learning to read and write, and today it is largely adult Indians who are illiterate.

Despite modest wants and unbelievable thrift, the mountain Indians are constantly on the verge of starvation. Failure of a crop means famine. Their diet consists mostly of potatoes, *oca*, dried

beans, and quinoa. *Oca* is one of the tuber-producing plants of the high Andes, grown long before Columbus reached the New World. Unlike the potato, however, which now is known over much of the earth, *oca* has remained in its highland home. The taste is similar to that of boiled sweet potatoes, or to a cross between that of green corn and pumpkin. (For an excellent study of *oca*, see Walter H. Hodge. Tuber Foods of the Old Incas. *Natural History*, 1949, 58, (10).) Quinoa is an ancient crop of the Andes and was regarded as sacred by the Incas. It can stand more cold than any other cereal. The United Nations Food and Agriculture Organization is now making a major study of this crop in the Andes to help reduce wheat and flour imports. Beans supply most of the protein. Meat is eaten only occasionally, and sweets rarely. Malnutrition is the rule, not the exception; since the sierra is one of the worst-fed regions in the world, sickness is widespread and a shocking death rate is prevalent among children, from 20 to 33 per cent dying before the fifth year.

Peru has yet to reach the minimum decent food standards set by the Food and Agriculture Organization (Hot Springs, W. Va.). Lima is consuming



FIG. 7. Tiny Indian fields separated one from another by stone fences. Such fields are not adapted to modern farm machinery; human muscle necessarily does most of the work here, as it has for centuries.

less than three fourths of this minimum, with the percentage in rural areas dropping to 42 per cent for Huánuco, and the national average being 50 per cent. To bring the national average up, Peru must produce 263,000 additional tons of grain; 111,000 more tons of vegetables; 277,000 tons of fruit; 180,000 tons of meat; 69,000 tons of fat; and 277,000 tons of milk. Peruvians still average fifty centavos a day for food, with the sol more than ten to the U. S. dollar. Many spend as little as ten centavos, now less than one cent U. S. (Shellaby, R. K.: SCIPA Means Food. *Americas*, 1949, 1).

Under such conditions it becomes imperative for the Indians to supplement their scanty living by engaging in home industries, by serving as carriers in cities and between regions not yet reached by truck or railway, by working as miners, by becoming transient laborers on the sugar, cotton, and rice plantations of the coast, or by actually moving to Lima (some fifty are reported to be doing this every twenty-four hours). The mountain Indian's health suffers when he remains at sea level for any considerable time, and all the supplementary measures enumerated here are but temporary palliatives; the real ailment (hunger) lingers on.

The problem produced by these conditions sooner or later will have to be met head-on, and it

must be met by the government. Unless these Indian farmers, who can scratch no more than a bare subsistence from their bleak sierra, are helped to become a productive factor in the country's agriculture, a really sound national economy cannot be developed. Farm practices must be improved, good tools supplied, transport facilities increased and improved, and, above all else, the system of land tenure altered. More than three million Indians in the sierra live by farming. Actually there is a relative surplus of population in terms of arable land. The more favored valleys are so completely occupied with fields that a larger population can no longer find support except by diminishing the per capita food supply or by part-time employment elsewhere.

The agriculture in the sierra being largely of the subsistence type, only an infinitesimal part of the yield of the soil ever reaches even a local market; 99 per cent is consumed on the spot of its growth. Thus Andean agriculture affects the outside world hardly at all; the Indians can live and do live without either exports or imports.

Sierra agriculture undergoes a progressive decline from north to south, a decline attributable to increasing latitude and altitude (7,000 feet in the north, 13,000 feet in the south near Lake Titicaca).

In the north, where the climate is less severe, the number of crops is greater—beans, corn, *oca*, potatoes, even some fruit; in the south, concentration is on the hardier crops—potatoes, *oca*, barley, and quinoa.

No discussion of mountain agriculture should fail to mention the terraces. At elevations of 5,000–1,000 feet, the terraces, or retaining walls, were built of huge rocks, unsquared but fitted together with precision. They transform even precipitous slopes into small level fields. All are centuries old, and some possibly reach back into the past for two thousand years. Though many have fallen into disuse, others still are yielding abundantly. Terraces enable tens of thousands of people to live in a milieu that in its natural condition could not support man agriculturally at all.

Unfortunately soil erosion is reducing the productive area. Countless slopes have been and still are being stripped of their precious topsoil and are horribly gullied. It is obviously in the longest-inhabited and the most densely populated areas that the largest portion of steep land has been pressed into service. Some of the unterraced but cultivated mountainsides are so steep that oxen cannot be used; only the hoe and the footplow can be employed.

In the high *punas* Indian shepherds pasture their sheep, llamas, and alpacas (Fig. 10). Their huts and corrals, built in the lee of the cliffs just below the edge of the plateau, protect humans and animals from the violent winds. These huts rank among the loftiest human habitations on earth. No cultivated fields are near them.

Most of the minerals, especially the metallic ones, are located high up in the Andes,† where the air is thin and only the "mountain men" can perform physical labor. The Indians are the only productive element in the sierra, both in farming and in mining. Nor is there any chance of their being replaced by human beings from any other part of the world. They are well acclimatized; their barrel chests house larger hearts and their blood contains far more red corpuscles for meeting the problem of oxygen deficiency than is the case with men at or near sea level. It is estimated that about twenty to twenty-five thousand Indians are employed in the mining industry at large. They prefer to work the soil of their farms, however, and partly because of this scarcity in labor there has been for some years now quite a lag in mine development. Some white men consider the Indian miner inefficient

and irresponsible, but others assert that he can become quite efficient.

One reason why Indians of the sierra are considered "unreliable" is their habit of leaving any kind of work when the time comes to harvest their own crops in the mountains. Everything else is of secondary importance! Of this, Carlos Monge, noted authority on the mountain Indian, says: ". . . but sooner or later he [the mountain Indian] returns to his place of origin where nature and the



FIG. 8. Farm children of the sierra. The majority of the people live under conditions of bitter and almost overwhelming poverty.

accustomed economy of his community furnish him with his ideal life conditions. These cycles are usually annual. . . . Thus he obeys without realizing it an ancestral biological law. Peruvian sociology must one day give to these facts their proper interpretation" (*Acclimatization in the Andes*. Johns Hopkins Press, 1948). Probably a hundred thousand men are involved in the seasonal labor migrations of the sierra to mines in the Andes and to haciendas in the coastal region.

After more than four hundred years of associa-

† The word "Andes" is said to come from the Indian *anta*, meaning copper, or metal in general.

tion with white men, the mountain Indian sees little that is good in most of them. From the conquest to the present moment, the white man has been an exploiter, and the government he represents is, in the Indian eyes, oppressive, dishonest, and discriminating.

The white man, for his part, regards the Indian as little better than a domestic animal; hence he is concerned neither with his abjection nor his needs. It is no exaggeration to say that the vast majority of the *haciendados* believe that their system of exploitation rests upon the continued ignorance of the Indian. They are, accordingly, unsympathetic with efforts promoting general public education. They fear that once the Indian learns to read and write, he will no longer be exploitable. Education is thus intermeshed with economics. The white man, too, often looks upon the Indian as a burden to the country and an obstacle to progress.

It would be erroneous, however, to believe that all whites at the time of the conquest, now, or in the intervening centuries have treated the Indian badly or have been apathetic to his interests. Many have tried to raise the Indian to the status of the Spaniard, but their efforts have been more or less futile. In the end, those who demanded great numbers of slave laborers won. Lewis Hanke, for example, believes that no nation in Europe, except possibly Portugal, took Christianity toward the Amerindians so seriously as did Spain (*The Spanish Struggle for Justice in the Conquest of America*. University of Pennsylvania Press, 1949).

The Alianza Popular Revolucionaria Americana (APRA) is opposed to the plantation system and wants the Indians educated, but thus far its accomplishments have been relatively unimpressive. Nonetheless, it has revealed potent possibilities as a generative force in any amelioration in the condition of or in any future liberation of the Indian. A fundamental aim of *Aprismo*, since its beginning in 1929, has been "education for the masses."

Pizarro and those who followed him tore the Indian social organization apart, and after four hundred years of disorganization the Peruvian government still has no plan. Lacking education, the Indians can take no part in public life. To make matters worse, there is probably no other important country in all Latin America where the colonial tradition survives as it does in Peru.

It has been pointed out that the amount of arable land in the Peruvian sierra is definitely restricted, and that for the most part the methods of farming are centuries behind the times (Fig.

11). It has been pointed out also that, in spite of ill-health, the population is increasing at an amazing rate and that the standard of living is dangerously low. Tyrannies have been heaped upon the Indian population ever since the days of Pizarro. When the dam breaks—as it most certainly will unless the upper class, the oligarchy, does something constructive—a terrific torrent may be expected to hurl itself forth. Nor will oratory and blueprints alone suffice; to date these have done little toward improving the lot of the Indian.



FIG. 9. Typical male costume. In the cold sierra, people dress to keep warm.

Even those whites and mestizos who sincerely desire to help the Indians seem unable to agree on a program. Some believe that the elimination of illiteracy is the number one problem; others that literacy is futile to those living in economic quasi slavery, that the problem is insoluble until the economic problem is first solved.‡ Yet, in essence, the "Indian problem" consists primarily of incorporating the Indian more completely into the nation and doing something about the problem of land. There can be no real political and economic stability in Peru until an honest attempt is made to bridge the tremendous chasm that separates the classes and the masses. To a degree, the

‡ The whites and Indians do not agree on the matter of the Spanish language. The Indian can see no useful purpose in learning Spanish except for those of his people who dwell near large cities.

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Over much of the sierra, one sees things that shock every sense of justice.

Democracy, real democracy, obviously will be a difficult goal to attain in a region such as the sierra, where the very foundation—a literate population—is lacking. Democracy and literacy go hand in hand. Dictatorships, both economic and political, breed and flourish on illiteracy. But even complete literacy would not automatically solve the problem. Social cohesion is unattainable so long as there exists this great landless peasantry. Land hunger and the most abject poverty make the mountain man a factor to be reckoned with. For centuries his spirit appeared to be broken; perhaps it was, and perhaps most of it still is. But though the Indian is silent, patient, resigned, he has a tremendous capacity for resistance. Indeed, passive resistance has been his most effective weapon for four hundred years against abuse, deceit, and exploitation.

During the past decade or so, the picture has been changing slowly but unmistakably. Roads have been penetrating the sierra—roads connecting it with the coastal cities, especially with Lima. These roads have tempted some Indians (when

free from debt) to leave the haciendas where they have been exploited. To the poverty-stricken peon, almost any city wage appears attractive.

Interested persons have been pointing out to the Indian that he should fit himself for leadership; that he should insist on a "new deal" in land distribution throughout the sierra; that he should not be denied access to schools and courts simply because he speaks his own language rather than Spanish. To date, the linguistic walls between the whites and the Indians are as solid and impenetrable as the stone walls of the great Inca fortresses and other buildings. According to law, each *haciendado* is obliged to operate a school for the children of the Indians who labor in his fields. In practice, however, few such schools are maintained. During the past several years the Peruvian government has provided a system of universal free public education for its rural population, but the number of qualified teachers still is pitifully small.

Dark storm clouds appear to be gathering over the Andes; lightning flashes sharply at times. The rumblings following these flashes seem far away now, but they are unmistakable, they are getting closer, and they are ominous. Over the Andes hovers the specter of agrarian unrest. Will the showdown come by *evolution* or by *revolution*?



FIG. 10. Grazing in the *puna*. The cold, disagreeable *puna*, characterized by perennial herbs, is a drab-appearing region which looks little different in summer or winter. It is basically livestock country. Animals in this view are moving toward a corral made, like nearly everything here, of stones.



FIG. 11. Agricultural methods consist of a combination of primitive pre-Conquest and colonial practices. These scenes show not the exceptional but the common farm practices. Above: Indians prepare the land for potatoes. Men break the ground with footplows, women and children turn over the clods. Below: Oxen hitched to the classic wooden plow. The plow is almost the sole animal- or power-drawn machine used throughout the greater part of the Indian country. It is employed in the preparation of the land, in planting, and in some cultivation.

Probably no one knows accurately at the moment the answer to this question. It is, however, within the power of the white overlords to settle peacefully. But by doing so, they would stand to lose financially. Will they (do they now) see the handwriting on the wall? They should meet the problem with intelligence, decision, and fairness, retiring gradually and thereby enabling the new order to develop slowly and securely. If they continue obdurate, it will be because too large a percentage of their number, consciously or unconsciously,

possesses the mentality of the encomendero. The Indian, on the other hand, is unmistakably emerging with sufficient vitality to give impetus to the recent movements for social reform, and he is engendering in the *haciendados* of the sierra an "ill-concealed fear of a general uprising." The mountain man is beginning to see the light even though as yet it be but a faint flicker.

PICTURE CREDITS: Figures 1, 6, 7, and 11 (right), courtesy Institute of Inter-American Affairs; Figures 3, 4, and 10, courtesy of Runcie; and Figures, 5, 8, 9, and 11 (left), courtesy of A. Guillén.

Reminiscences of Professor Osborn

ROBERT CUSHMAN MURPHY

Dr. Murphy has been chairman of the Department of Birds at the American Museum of Natural History since 1942, and Lamont curator of birds since 1948. His last appearance in these pages was in August 1949, when he reported on the Seventh Pacific Science Congress.

IT IS fifteen years since Henry Fairfield Osborn died. Like men of kindred attainment, he has been abundantly memorialized in the journals of science and the proceedings of academies. His contributions to learning, to living, to civilization, are an open book for all who care to read. And yet nowhere have I seen him depicted quite as I remember him.

Of his friends, I am among the least qualified to set down any formal account of his traits or accomplishments. My appreciation of him is that shared by many associates, particularly by those who followed his leadership on the staff of the American Museum of Natural History. Except in the field of conservation of natural resources, I have not been even a minor contributor to the branches of knowledge in which he attained eminence. All that I can claim is an exceedingly junior, yet increasingly close, relationship with him from about 1913 until the date of his death. My present role therefore corresponds with that of the "sub-sub-librarian" who filled a few pages preceding Herman Melville's biography of Captain Ahab and the whale.

During a score of years I served on numerous committees that dealt with matters close to Professor Osborn's heart. I had the honor of representing him on various occasions, such as making fighting speeches during the conflict between the evolutionary idea and so-called religion that was precipitated by the Scopes' trial in Tennessee. On his behalf I read his essay at the dedication of the bust of Louis Agassiz in the Hall of Fame. I was a regular attendant at meetings of his Journal Club in the Osborn Library, at which members of the Museum staff and invited colleagues presented reviews of scientific literature, and to which he

usually contributed a stimulating share of the discussion. By his designation I played a part (memorable to me) in the hospitality to foreign men of science whom he entertained while they were visiting New York, these including the oceanographer Prince Albert of Monaco, Dr. Fridtjof Nansen, and others. I was present at the spontaneous festival, presided over with such mellow charm by the late Professor Edmund B. Wilson, which a company of Professor Osborn's friends created on his sixtieth birthday, at Garrison, in 1917. And, finally I had the good fortune to be an American delegate to the annual meeting of the British Association for the Advancement of Science when, in early August 1926, he received the honorary degree of Doctor of Science from Oxford University.

There is yet one other tie, which is quite a posteriori but of which I like to think he would approve. Upon my daily entry through the first floor of the Roosevelt Memorial wing of the museum, to which Osborn as president gave the labor of fifteen years, I pass Joy Buba's bronze portrait of his countenance. Then I salute him not only as the outstanding paleontologist of the Golden Age of that discipline, as a biologist of magnificent breadth, a seer, philosopher, and friend, but also as the ancestor of an F_3 generation in which I take a special interest—in other words, as the great-grandfather of some of my grandchildren!

From such a miscellaneous background, rather than as a colleague who toiled with him in a particular vineyard of science, I shall attempt to record a few reminiscences that will depict Professor Osborn less as a biologist, conservationist, and educator than as a personality who left an indelible stamp upon all with whom he came in contact.

Henry Fairfield Osborn, as noted by most of his biographers, was a man who liked big things. The statement has both a literal and a deeper meaning, because, just as he preferred to study the fossil bones of giant mammals and reptiles dating from former ages in the history of the earth, so also he devoted his administrative energy toward the development of ideals that were sometimes too large even to be seen by men of lesser mental and spiritual stature. His vision pierced the veil of the distant future, and, as worded by his younger colleague Dr. G. G. Simpson, "he planned as if he were to live forever." In every phase of his life he expressed the desire to excel, a standard which he held up equally for himself and for all who worked with him. In the case of a man who produced an output of nearly a thousand publications, besides founding, building, and directing several scientific departments or entire institutions, it goes without saying that so much could be accomplished only by supremely effective budgeting of his time. His self-imposed regime was, indeed, a Spartan one throughout the whole of his adult life, and he stood rigidly by his slogan to "do the important thing first." Nevertheless, success was not achieved at the sacrifice of human values, because he invariably included within his schedule reasonable freedom to be devoted to carefree association with Mrs. Osborn, their children, and other members of the family.

It was no doubt to the extraordinarily well-ordered regulation of a life which had led to such attainment that William Church Osborn referred at the sixtieth anniversary luncheon, when he called his brother a "self-made man." Superficially, the term might sound slightly ironic as applied to an individual born to every advantage offered by wealth and influence. It must be remembered, however, that no scientific antecedents are to be found in the family tree of Professor Osborn. The pulls of precept and example were all in other directions. Countless less arduous paths toward usefulness and high success lay open to him from his boyhood days. His choice involved penalties which he accepted happily for the sake of the goal he sought.

In the light of the foregoing suggestions regarding Professor Osborn's awareness of his own responsibilities, it is not surprising that he was customarily an autocrat of much more than the breakfast table. His own children make no apology for remembering him as a benevolent despot both at home and abroad. Mrs. Osborn, who was always closer than anyone else to his every thought and act, sometimes addressed him as "Cheops," whom his

elder son recalls as "one of the foremost WPA builders of ancient Egypt." Sure of himself, eternally optimistic, confident of the success of many things that his associates considered impossible, Professor Osborn in his administrative realm was not always too tolerant of opposing views, or even of opposing votes, and in many instances his own faith and relentless drive were ultimately justified. He arranged his own time-scale for the incubation of cherished plans, and it usually proved quite as difficult to accelerate his approval of particular steps as it would have been to delay them after he had once arrived at his decision.

I well remember an instance in which I was personally involved as secretary of a responsible committee of which Professor Osborn, as president of the museum, was chairman. The other members, numbering seven representatives of scientific departments, had met repeatedly for deliberation, had reached conclusions and prepared agenda. All that remained necessary was the approval of the president, but for reasons best known to himself this was not forthcoming, and days lengthened into weeks while one small item of museum progress was apparently held in abeyance. Encountering Professor Osborn one morning on his way through the long corridor leading from the elevators to his sanctum in the Tower Room, I determined to corner him and to obtain the only word that could lead to action. I had not seen him for a considerable period, and so, as always, he stopped and grasped my hand. As soon as the amenities would permit, I popped the question of the committee's business. His response was to look me straight in the eye and inquire: "How are those fine boys of yours?" To such imperturbability only one kind of reply was possible. The upshot was that he led me to his office for the purpose of receiving two books, which he inscribed as gifts to my sons. One of them was *Moby Dick*, and I forgot the other, but the work of the committee remained for the time in *statu quo*.

It is not farfetched to say that the riposte used by Professor Osborn on this occasion revealed another trait of his temperament, namely, his interest in children and his sympathy with all that youth implies. His grandchildren were an outstanding passion of his latter years, when, indeed, he used to gather them at his home for an annual springtime visit, which he called "Creative June." He took also a perennial interest in the children of all his associates and assistants, and he carried the patriarchal conception into a still wider field by referring to his former students at Columbia University as his "biological sons," and to *their* students as his "bio-

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logical grandsons." He had founded at Columbia two notable graduate courses, known respectively as "Evolution of the Vertebrates" and "Mammals, Living and Fossil." These had descended after his retirement from teaching to the charge of one of his foremost "biological sons," Professor William K. Gregory, who for thirty-six years continued them and steadily expanded their content in his laboratory at the American Museum of Natural History. It was a keen and vigorous group of young men and women on the threshold of their scientific careers who worked in these courses during the opening years of the first World War, at which period I had the good luck to be one of the "grandsons." Fellow-students included the late Dr. G. Kingsley Noble, of the American Museum; Alfred S. Romer, now professor of zoology at Harvard; A. Elizabeth Adams, professor of zoology at Mount Holyoke; Charles L. Camp, professor of palaeontology at California; and several more who have reached the pinnacles of their calling.

Professor Osborn's interest in this continuation of his own pioneering never wavered. He made frequent inquiries, and occasional visits to the laboratory, and he took pains to examine the notebooks of the students. In communications now before me, dated in June 1916 and 1917, his bold and sweeping penmanship expresses to his successor his "felicitations on the splendidly thought out course, the transparently clear lectures, and the admirable record in text and drawings." The last he was well equipped to judge because of his own facility in sketching, an ability which, according to so able an authority as the late Dr. Charles B. Davenport, is characteristic of most biologists.

The effect of such earnest solicitude was by no means lost upon Professor Gregory's students, who could thus look back through the academic generation of their immediate preceptor to a master who had been a pupil of William H. Welch, Francis Maitland Balfour, and Thomas Henry Huxley; who had met Charles Darwin and had collaborated in publication with Francis Galton!

Professor Osborn's interest in his own former graduate courses was akin to his sympathy with the labors of all members of the scientific coterie under his leadership. It seems to me that I never distributed a paper of my own, large or small, from the *Bulletin of the American Museum of Natural History* or from any other journal that did not bring me a few lines of commendation from the president. He was invariably one of the most encouraging of men, quick to see merit in the ideas of others and equally so to point out paths that might lead to still richer ends.

If, as I have suggested above, Professor Osborn did not always welcome opinions that differed from his own in the administrative sphere, I should hasten to make clear that in scientific research no such attitude was ever apparent. Here he not only tolerated but actively encouraged criticism of his own interpretations and conclusions. If differences of opinion led to an impasse, Professor Osborn was meticulous in stating the opposing view, and as quick to acknowledge the aid of a collaborator as though the conclusions had been joint and harmonious. He listened with an entirely open mind when G. K. Noble, at a meeting of the Journal Club, suggested as a result of certain endocrine experiments that the evolutionary significance of the orthodox Cope-Osborn theory of tritubercy in mammalian molar teeth might have to be weighed anew. He never forgot human liability to err, and never expressed either objection or pique when even his youngest fellow-workers disagreed with him on public platforms.

I recall one incident which has not previously been recorded. At a meeting of the New York Academy of Sciences, Professor John C. Merriam, then of the University of California, demonstrated a remarkable series of skulls of Pleistocene sabre-toothed tigers, obtained from the famous Rancho La Brea tar pits. A point especially stressed by Professor Merriam concerned two distinct types of cheek teeth exhibited among these fossil jaguarlike cats. Merriam explained the difference as one of individual variation in a single species, but Professor Osborn, while discussing the problem at some length, held that the tooth distinction was more likely to mark an evolutionary divergence in which two different phyla were already represented among the series of some 800 sabre-toothed tigers studied by his Californian colleague. Merriam closed the session by producing one or more complete and perfect skulls which had type A cheek teeth on the right side of their jaws and type B on the left! To the audience, which included many laymen as well as naturalists and biologists, this thrust clinched the argument in a way that proved both convincing and amusing. A titter swept through the room, culminating in hearty applause led by Professor Osborn himself.

The Professor was a man of such unvarying dignity and serene self-confidence that merriment could hardly be called a feature of his daily life as seen by his colleagues. Nevertheless, some of us caught glimpses of the lighter side often enough to credit the rumor that he could be wholly jovial during hours of relaxation with the members of his family, particularly with those of the youngest gen-

eration. My own recollections of Professor Osborn in a jolly mood center chiefly around the Christmas holiday luncheons at which he was host to the Department of Vertebrate Palaeontology and a few other guests. On these occasions he could be delightfully whimsical, as for example in a running lecture and demonstration on such a serious matter as correct technique in carving a turkey. On this subject he was, I believe, the founder of a new scientific and gastronomic school, the sound tenets of which I have since had occasion to test and prove on many wild South Carolina examples of the magnificent fowl that ought to be our American national bird. The Professor held, with all the solemnity of Dean Swift discussing the Lilliputian problem as to which end of a boiled egg should be broached, that a turkey must properly be carved *across* the *pectoralis major*, rather than sliced downward from the keel of the sternum as though it were a mere chicken!

Although for Professor Osborn the business of living was necessarily minutely organized and complicated, he exhibited a refreshing gratification in experiences of the simplest sort. One November morning, a migrant saw-whet owl, the smallest member of its family known in the New York region, was discovered resting sleepily on a window grill of the first floor of the museum. Several times during the day I went down in the elevator and out the Seventy-seventh Street entrance in order to inspect at close range this dozing little visitor from the north. Late in the afternoon, I telephoned the president and invited him to accompany me on a final call. Somewhat to my surprise, he accepted, and within a few moments the blinking saw-whet and the Professor were looking into each other's eyes at a range of three feet. Then the incredible happened, for the owl opened wide its bill, made a slight gagging gesture, and ejected at our feet an oval pellet comprising the indigestible ingredients of the previous night's dinner. "One touch of Nature makes the whole world kin," quoted the fascinated Professor, while I picked up the pellet for later analysis and identification of its contents. I could think of no more appropriate comment than the time-honored limerick:

To a hen said Henry Ward Beecher,
"You are surely a wonderful creature!"
The hen, just for that,
Laid an egg in his hat,
And thus did the hen reward Beecher.

Never did the delighted President seem to fill better the role of his title as transliterated into Chinese during his visit to the Far East at the time of our

Asiatic Expedition—"The Presiding Magistrate of the American House of Heavenly Creations!"

I trust that such trifling incidents of a great man's career will serve at least to indicate the kindness and sympathy that underlay his generally austere aspect. This was surely sensed by all who were brought into intimate contact with him. The writer of an editorial published in the New York *Herald Tribune* after his death rightly called him so kindly a man that it grieved him even to tell the antievolutionists that they were wrong. During the course of the violent but brief flare-up that followed the Tennessee trial, Professor Osborn said and wrote: "The earth speaks to Bryan, but he doesn't hear a sound." That statement probably represents the full extent of his "abuse" of any antagonist. He stood all his life for complete freedom of expression in research and education, and he would have been the last to deny the same privilege to even the most benighted of his adversaries.

Kindness and fair play are closely related, or they may be kindred aspects of the same trait. Professor Osborn showed a never-failing loyalty toward all who worked with him in furthering the causes to which he had dedicated himself. Many of us recall a mayor of Greater New York who, by his own subsequent admission, came into office as guiltless as a newborn babe of any knowledge of natural history or of most other aspects of modern science. This man, who is generally credited with having been upright and well-meaning, is reported to have said, at the time of his first consideration of the budget, that the American Museum had more curators than would be needed to cure all the ills of the city and the nation together. His attitude toward what we regarded as our own institutional deserts, no less than the city's gain, was, to say the least, inhospitable.

Upon this mayor, Professor Osborn went to work with his customary directness and grave courtesy. The result was a complete change in the attitude of the chief magistrate, who gradually became enlightened to the point of regarding the museum as an indispensable part of the municipal educational system. At the end of two terms, popular sentiment switched against the mayor; he and the members of his organization were voted out of office, and so, some did not hesitate to say, His Honor would be of no further use to us. But it was in that very period, namely, between election day and the retirement of the administration from City Hall, that Professor Osborn chose to tender a luncheon at the museum to the candidate who had failed in the effort to succeed himself. The affair was attended by a large proportion of the museum trustees and a number of distinguished guests, and Professor Os-

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born expressed the warmest possible appreciation of one who had become a convert to our museum faith.

In August 1926, my family and I reached Oxford in what our English hosts naïvely called a "hot spell," but which seemed to us good summer weather for exercise. Professor and Mrs. Osborn, and his secretary, Miss Florence Milligan, were already there for the 94th annual meeting of the British Association for the Advancement of Science, of which the Prince of Wales had been chosen president. Throughout a never-to-be-forgotten week, Professor Osborn was in top fettle, particularly at the *conversazioni* and other social affairs, when he could hobnob with such old cronies as Sir Oliver Lodge, and Professors Bather, Poulton, and D'Arcy Thompson. I regretted very much that he was absent from a session during which Julian Huxley read a paper on differential growth, or heterogony, purporting to knock the orthogenesis completely out of the titanotheres. We had a conviction that Professor Osborn would have risen mightily to the defense of his own prize cattle!

The climax of the visit came, for us, in the forenoon when the Professor received the degree of Doctor of Science, in the Sheldonian Theatre. The Latin ascription of Dr. A. B. Poynton, public orator, contained words "that would have made Quintilian stare and gasp." It was printed in a single paragraph and was read almost in a breath—like clearing ditch, bank, and hedge in one jump. I append a translation:

Not only is the New Atlantis rich in remains of ancient animals but no city is more replete with paleontological monuments than New York, in the Museum of which this man, its head, has so wrought that this depository of natural objects has been of service to very great assemblages of students, particularly for those pursuing specialized studies in Geology. This most skilled man, indefatigably devoted to the organization of expeditions, has gathered together, illustrated, and arranged a great wealth of wonders in Zoology and Paleontology. He has made himself master not of a single reptile alone but, having read the auspices, he rejoices in the flapping of vertebrate wings, the magnificence of tails, the enormousness of their bodies and the insignificance of their brains. From the amphibians he has pursued them earnestly through eighteen orders over an immense period of years; and the Dinosaur was disclosed to us by the fates. But why should I speak of the Dinosaur? There are among us Oxonians certain Magdalenians, whose President once (as was recently learned) "crushed with a rock the brain" of such a monster in a common cave. But I return to our guest: turning from reptiles to mammals, he has investigated the Titanotheres and has taught us how the horse, progressing gradually,

**Saxo cere comminuit brum.* The Latin whimsicality will be recognized as an example of *tmesis*, in which the verb splits the noun, as the rock the brain.

has exchanged his lowly stature of a little fox for the beauty of an Olympian victor. And these matters concern things which are within the province of Epimetheus. Besides, he has explained in many writings by what steps the human race, aided by the shrewdness of Prometheus, has emerged from the caves into the light and culture of life. Nor has he merely placed before us vividly the appearance of natural objects but, by calling into play his intellectual powers, he has adorned the subtlety of Democrats with the eloquence of Protagoras. I therefore present to you this ornament of the Transatlantic Republic, Henry Fairfield Osborn, Doctor in Science of Princeton University, Honorary Doctor of Laws both in the same university and in Columbia, Honorary Doctor in Science in our own Cambridge University and in Yale, chosen among our foreign members of the Royal Society, Professor of Research Studies in Zoology in Columbia University, in order that he may be admitted to the Honorary Degree of Doctor in Science.

Professor Osborn remained in harness to the end, for he continued steadily to work on the vast *Proscidea* monograph after his retirement in 1933 from the presidency of the museum. He passed away while he was actually at his desk in his home, Castle Rock, which looks up the curving Hudson toward Storm King. To the last he retained his zest for life and youth and sport, together with much of the elasticity that enabled him to accept new scientific ideas. Freudian psychology was the only emerging method and system of knowledge, so far as I am aware, upon which he looked askance. Few would deny, also, that he had been led somewhat astray in his views on superior and inferior "racial" categories of modern Caucasian man.

Throughout his career he fought for the cause of enlightenment, spreading his energy over many tasks which he recognized as parts of a whole, but upon any one of which a lesser man would have been content to rest his laurels. Singleness of purpose seemed to be his talisman. With admiration, almost with despair, his associates saw how in well-ordered days he rigorously assigned each task its place, sifting choice and impulse through his long foresight.

As a biologist, the discovery of new principles was the chief end of his research. A colleague has written that "even his errors served as guideposts for those who followed." As a vertebrate paleontologist, he was first a prophet and then a demonstrator of a radiating system of life which, in ever-ascending stages, had peopled the earth before the coming of man. As a conservationist, his endeavors as a member of twenty organizations devoted to the protection of natural resources went far toward saving the bison, the redwoods, and much else, as well as toward ameliorating the plight of fur bears and other threatened mammals.

As an educator, his influence can hardly yet be

measured. For that elusive figure, the "intelligent layman," he reconstructed the pageant of ten thousand human generations stretching back beyond Greece and Egypt, generations of which the elder historians had scarcely dreamed. His aesthetic sensitiveness led to new standards of beauty in the exhibits of the museum, at the same time only enhancing its security as a temple of truth. He carried many profound discoveries of a scientific epoch into homes and schools throughout the world, changing "dinosaur" from a high-brow to a household word and making Mesozoic dragons almost as familiar to children as the creatures of Noah's Ark.

Magna opera Domini, exquisita in omnes voluntates eius is the motto of the American Museum of

Natural History. These symbolic words likewise express the spirit in which Professor Osborn, so long the guiding genius of the museum, faced life. Always a scientist of the first water, he did not fail to be first also in the ranks of citizenry; with his head among the clouds, he kept his feet upon the earth. Tolerant even in the face of intolerance, reverent toward all things that any man respects as sacred, exemplifying in his own home a conduct which all admire, he could fairly ask why man had been uplifted to Parnassus unless he were capable of rising still higher. Perhaps no teacher of our time has done more to convince his fellow-men that truth is never to be feared, for it can lead only to nobler testaments.



MIRAGE

Clear to the mind, complete as a shell in the hand,
The green sea stretches in a blaze of sun
With afternoon's hot quiet over the sand,
Gulls in the air, gold light on every one . . .
Their sharp wings weaving together water and land
With a frieze of clouds above the crescent beach.
The rhythm of our thoughts, implied or spoken,
Rocked with the waves. Below us at tide's reach
Scallops and weeds, a chain with few links broken.
Only a part of what we said was speech
Caught with the wind into that glittering air.
Dark on the horizon, gradually twin ships
Curved up from unknown waters to appear
Precisely moving at our pointed finger tips,
Tiny, far off, never to draw near.
Not for me, ever, to comprehend, though you explained
Four sails repeated flawless on the sky,
And while we watched and heard the singing wind
That urged the vessels, one vanished utterly . . .
Night came with freshening salty change, and far behind
Us in the town we heard the bells recall
The hour. Remembering the phantom in reverse
We cried together, "It was not there at all."
Huge in the dusk the true ship held its course.
We turned in silence from the high sea wall.

EDNA L. S. BARKER

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Combinationalism versus Theorism

J. A. RAFFERTY

J. A. Rafferty received his B.S. from Harvard in 1943 and his M.D. from Rochester in 1946. For a year he taught medical statistics at Rochester. Since then he has been chief of the Department of Biometrics, USAF School of Aviation Medicine, Randolph Field, conducting biometrical research, and, with his staff, serving his institution with statistical consultations. The opinions represented herein are the author's own and not necessarily those of the USAF.

THE trend toward hyperspecialization in the professional fields of medical research has produced a contrasting picture between the classical and the modern researcher. The former was a jack-of-all-trades, a physiologist, say, a biochemist, a physical chemist, a statistician, and an instrument-maker. He comprehended sufficient basic theory and was manually adept enough to implement theoretically and mechanically his new ideas for experiments. The latter is a walking encyclopedia of what-has-been-done and what-is-known in his field. From these historical and technical facts he constructs new experiments. In order to implement his new ideas he must often consult specialists in theoretical subjects and in instrument design. A theoretical subject which supports a large variety of experimental fields is biometrics. The recent popularity of statistical methods, the trend toward the use of mathematical models in biological theory, and the usual deficit of statistical training in the education of medical researchers account for the apotheosis of biometrics in present-day medical research.

The philosophical implications of the trends outlined above are not just mental pabulum for research analysts. They are responsibilities of biometricians associated with experimentalists. Their subtleties become frank policies in departmental administration. The modern researcher is often so steeped in the details of the history and techniques of his field that his inspiration for new ideas is inbred. To outbreed he must—and does if he is at all alert to his shortcomings—interchange ideas with colleagues in other fields. Repeatedly this technique of outbreeding fails to produce an organic research structure. The complexity of an organic structure is acquired; the evolutionary potential is not.* The outbreeding is a sterile conjunction of ideas and

techniques from related fields—"combinationalism." The biometrician may be implicated as either the conjoiner or the conjoined.

The serious biometrician will want to consider the products of his activities before undertaking them. Thus, the biometrician naturally comes to make value judgments on the research proposals and activities of his associates. He will want to commit his intellectual and computational services to activities that in the long run will maximize scientific achievement. He will want to deploy his forces strategically, since his forces will always be finite. There follows, then, a more thorough analysis of combinationalism, an introspective account of biometrics, and some portents of heuristic synergism between biometrics and medical research through theorism.

Combinationalism is an affliction of research due in part to luxury. Money allocated to special research areas begets specialized literature. To be considered properly educated, a researcher must comprehend (or pretend to comprehend) all specialized literature that falls under the currently accepted definition of his field. Having consumed all his time keeping informed in his field, the researcher has no time for thinking. His "new" experiments are permutation-combinations of old ones, or at best are exploitations of old principles with novel experimental conditions. Financial support can be obtained from persons, industries, or agencies of the government that have vested interests in the results. More luxury, more factual results, and the combinational spiral whirls on!

Not all of this is bad. Society has recently given recognition to the scientific method as an efficient technique for acquiring knowledge. A young scientist can now earn a decent living. Men of established scientific reputation can acquire a wealth of personnel and apparatus merely by asking for it. Curiously enough, however, the good name of science, actually due to scientific discovery, is being

* There are, of course, some prominent exceptions to these statements. In examining exceptions, however, one should suspect genius in one or more of the researchers.

sustained by the wholesale application of the scientific method to everything in life. A doctorate degree in science is synonymous in the public mind with a license to practice the scientific method. Public support of scientific research, which is physically limited by the production of economic wealth, may wither in the intellectual drought of combinationalism when the expense of research becomes overwhelming. Society may well forsake the scientific method as an efficient (or even credible!) technique for acquiring knowledge and turn to demagogic exponents of extrasensory and metaphysical techniques for acquiring knowledge.

Medical research is guilty of combinationalism. In the absence of comprehensive scientific theories in biology and medicine, under which quantities of facts could be subsumed, a student in medicine or biological science is required to absorb the bare facts and arrange them in his mind to the best of his ability, inconsistencies and contradictions notwithstanding. Being well educated (having absorbed many facts), he undertakes research. In keeping with his training, he considers it research, say, to combine in an experiment a modification of an old technique and a different (not reported in the literature) animal species. Or he may endlessly repeat old techniques on a series of new chemicals injected into animals of the same species. He may even repeat or modify classical experiments which led to inconsistencies in the literature, in an effort to resolve these inconsistencies. To support his efforts he can easily find some industry, pharmaceutical house, or agency of the government that will subsidize his laboratory, provided he advertises that applications are found in "antibiotic therapy," "cancer," "heart disease," "atomic medicine," "aviation medicine," "industrial hygiene," "personnel psychiatry". . . .

The national scene in medical and related research is even more foreboding. Conclaves of archscientists, some in government employ, others consultants to the government, establish a taxonomy (with code numbers for recording on punch cards!) of research projects. The taxonomy not only is in the style and tradition of combinationalism, but also imposes priorities and preferences. In view of the whole-scale government support of medical research, the pattern of combinationalism will be not just a hereditary characteristic of medical education but an indelible impress.

Certain characteristics of data which recur in combinational medical research tie contemporary biometrics to contemporary medical research. (1) Quantities of facts of experience are recorded and tabulated. Factorial classifications of these facts

parallel the combinational design of the investigation. (2) Empirical results *per se* are regarded as experimental proofs of simple hypotheses. Such hypotheses are highly specialized and often have little or no relation to broader, more general hypotheses. (3) Many experimental variables are measured simultaneously on the same organism. Multiplicity of measurements, an American tradition in medicine and physiology, is often a frank outcome of the combination of known techniques with bigger and better apparatus. (4) Large series of cases are collected or large numbers of experiments are conducted in order to put the force of large numbers behind the credibility of results. Applied medical research, which always bears the stigmata of combinationalism, requires statements of results which are trustworthy "beyond all reasonable doubt." (5) Standard mathematical models are used to rationalize dynamics and mechanisms. These models are seldom consequences of comprehensive general theories; they are often easily manipulated functions from standard mathematical references.

As medical research expanded rapidly along traditional lines during the past decade, an interstice of applied mathematics appeared in the research structure, delineated by recurring problems in handling data. Biometrics moved into the interstice and became a part of the research structure. The results were excellent, at first. Statistical machinery was able to cope with the mechanical problems of recording, sorting, classifying, and tabulating volumes of medical facts. Statistical techniques of experimental design were easily adaptable to analyzing empirical results on isolated experiments. Certain multivariate analysis techniques developed by statisticians in other fields were brought to bear on the multiple measurements of physiological experiments. The mathematical theory of testing statistical hypotheses was applied to the estimation of sample size in series of cases and repetitions of experiments, and to the assignment of a credibility measure to statements based on these experiments. Finally, ready-made mathematical models for describing biological dynamics and mechanisms were lifted from general statistics and handed to medical researchers on a least-squares-plated platter.

An introspective view of the success of biometrics, however, reveals several significant trends, some of which may be alarming to research analysts and serious biometrists. (a) Into the chaos of combinationalism in medical and biological research, biometrics, via standardized experimental and decision-making techniques, has introduced an

illusion of order. (b) The high desirability of good statistical treatment of data has often caused the mathematically untutored experimenter to make the biometrician an intellectual crutch. The biometrician is embarrassed. His conscience is troubled if he is at all honest intellectually, for the biometrician can hardly be a specialist in every field represented by his clientele. (c) Biometrical methods are still far behind those needed to construct and analyze satisfactorily some of the simpler medical experiments. Too much biometrical analysis is done with tongue in cheek when it comes to bridging the gap between statistical theory and biometrical practice in medicine. The intuition of the biometrician must be evoked all too often. (d) Applying known biometrical methods alone, the intellectual and machine man-hours required for the statistical design and analysis of an experiment are often equal to or greater than the man-hours consumed in conducting the experiment. Hence there is the implication that one half of the resources of a medical research center should be biometrical, a situation which would ordinarily be impractical and undesirable.

Biometrics has abetted combinationalism in medical and biological research by encouraging researchers to continue combinational tactics in research design and to rely on statistical rigor for the elevation of the intellectual quality of the research results. Throughout the assemblage of biometrical methods lies a core of consistency and generality due to the theory of mathematical probability. Although mathematical probability ordinarily concerns only the statistical hypotheses of an experiment, its intellectual charm has all but bewitched many researchers to the point where they have accepted statistics in lieu of comprehensive scientific theories. This acceptance could be condoned were mathematical probability an integral part of the biological and medical hypotheses.

Applied medical and biological research, a certain amount of which is necessary to anchor research to practice, requires the support of large-scale biometrical operations. Applied medical research centers have highly specialized staffs; they produce large amounts of survey records and experimental data in exploiting known principles; they seek definitive results. The proportion of the total effort invested in biometrics within a given institute is arbitrary and should be balanced against other activities in accordance with the research objectives of the institute. Actually, the essence of a large part of the applied research is a mixture of research direction and statistics, the former to set the objectives, the latter to effect them economi-

cally. Thus, up to 50 per cent of total research effort could be invested in biometrics (*vide supra*), depending on the degree to which the institute desires that data be "statistized." If biometrics is less than 50 per cent, the chief biometrician, the research director, someone, must make value judgments on each research project of the institute and allocate an appropriate amount of biometrical services to it, realizing that a complete job in all cases is not within physical capabilities.

At this point it might be well to review the functions of biometrics in a modern medical research center. They are three: collation of historical facts, design and analysis of surveys and experiments, and biometrical research. Relevant to the objective of the applied research program, whether it concerns cancer, tuberculosis, industrial medicine, or mental health, masses of heterogeneous recorded facts usually exist in archives of one sort or another. Rendering information from these archives that can help guide the research program is part of biometrics through the tradition of biostatistics. Statistical reports and digests, with tables and graphs, are the numerical results of the rendering process (e.g., incidence of morbidity and mortality, historical series of population, and health data). In designing and analyzing surveys and experiments, biometrists borrow statistical techniques from all fields of statistics. Most characteristic of this phase of biometrics are the general use of mathematical probability in describing the course of sensory data in scientific work in many different fields and the attachment of probabilities to conclusions and decisions based on sensory data. Biometrical research may take the form of biomathematics or applied mathematical statistics. Often new mathematical statistical problems arise in consultations with experimenters on experimental design. Biometrical research attempts to solve these problems or to formulate them and bring them to the attention of mathematical statisticians. Certain mathematical models for common biological dynamics, mechanisms and operations have become conventional subject matter for biometrical research: bio-assay, population attrition, morbidity expectancy, physiological functions, and genetics.

The large-scale recording, tabulating, and computing operations of biometrics in modern medical centers have given rise to the necessity for large biometrical staffs. More efficient mechanization of these operations could in part remove the threat of imbalance among research activities due to a ponderance of biometrical services. The current requirements of biometrics have outstripped the speed and versatility of conventional recorders,

tabulators, and computers. Perhaps medicine, through biometrics, will stimulate the development of a new line of rapid, versatile recording-computing machines. Were such machinery available in medical centers, the following obvious requirement of medical research could be satisfied: Information should be maximized. In practical terms this means that the biometrics department of a medical research center could have storehouses of facts for rapid conversion into information, once relevance had been established by the research staff of the center; that analyses of variance, factor analyses, bio-assays, multivariate L and T^2 tests, multiple correlations and regressions, curvilinear least-squares fits, and other tedious techniques of experimental analysis could be routinized with dispatch; that biometrical research could employ high-speed random samplers and computers for solving certain difficult equations and for finding empirical random sampling distributions of new statistics.

Biometrics itself has not escaped the contagion of combinationalism. Stimulated by requirements for the analysis of combinational medical and biological investigations, biometrics and associated applied mathematical statistics have undertaken research to satisfy these requirements. In the process, old principles have been used and reused for specialized applications. Analysis of variance in experimental design has been exploited to great detail. Normal probability theory has been applied and reapplied to situations wherein normality is an obvious fiction. Multivariate analysis has expanded along established lines to the point where computational labor often outweighs utility. The computational labor of bio-assay and curve-fitting has become almost prohibitive for the serial analysis of daily experiments. Many techniques have been developed for specific situations, each technique with its own reference tables. A practicing biometrician needs a five-foot shelf of statistical tables and another five-foot shelf on how to use them.

The mutual stimulation and development of biometrics and medical research, be it good or bad, has been synergistic. Medical research specialization led to combinationalism. Combinationalism in large-scale applied medical and biological research invited large-scale biometrical operations. Biometrical operations stimulated applied mathematical statistics to produce more biometrical techniques. These specialized techniques encouraged further combinationalism in medical research. Mathematical probability in biometrical operations became a generally applicable principle. But along with applied mathematical probability came a colossus

of mechanical technique and computational labor. At every point in this development spiral the whole of biometrics and medical research has been greater than the sum of both taken separately.

The phrase "development spiral" is used in contrast to the phrase "evolutionary potential." A previous paragraph expressed the view that combinationalism may produce superficially the complexity of an organic structure, but that combinationalism does not possess the evolutionary potential of an organic structure. In a stable milieu, specialization and overspecialization lead to comfort, that is, adaptation to very detail of the environment. But sudden shifts in the milieu bring catastrophe to overspecialized structures. What guarantee is there that the conceptual surroundings of medical research will obtain, even for a decade? What fluxions in epistemology may deal a catastrophic blow to applied mathematical statistics? Successful organic structures carry within their complexity the ability to adjust to shifts in the milieu. A structure of human endeavor must also carry evolutionary potential within its complexity to be successful in social and intellectual evolution. Thus it behooves scientists, such as medical researchers and biometricalians, if they have any faith at all in man's control over his destiny, to assume responsibility for the evolution of science. This may mean serious introspection and philosophical writing, participation in national politics, membership in government research agencies, or education of nonscientists and young scientists in the social and intellectual impact of science. In any event, the guise of combinational complexity should not conceal an evolutionary impotence.

The synergism of biometrics and modern medical research already exhibited in the stochastic sequences of combinationalism can perhaps be diverted to the directed sequences of theorism. Theorism is not an antidote for combinationalism. It is, however, a philosophy and technique of research design antipathetic to combinationalism.[†] Moreover, within theorism lie the germs of evolutionary potential. Theorism advocates the construction of a research program on a comprehensive general theory. Bases for the general theory may be purely postulational or a mixture of postulates and scientific "truths." Research projects in the program are details, each relevant to a logical (in formal sense) consequence of the general theory.

[†] The popular terms "applied research" and "fundamental research" have been avoided. They are not contradictory to, nor completely consistent with, the terms "combinationalism" and "theorism." The popular terms refer to objectives; whereas the new terms refer to designs. Furthermore, the popular terms are almost meaningless because of widespread use in various contexts.

Thus, there are no arbitrary boundaries on the types of projects which may be performed in the program; there are no distinctions among physiology, biometrics, biophysics, immunology, pathology, clinical medicine, et cetera.

Theorism, it will be observed, presents some formidable intellectual, organizational, and social hurdles. Who shall formulate general theories and how shall they do it? How many logical consequences of a given general theory should be investigated experimentally? How can public support of research be enlisted without semantically loaded slogans such as "cure for cancer," "care of the flyer," or "painless childbirth"? A compendium of rules for implementing theorism will not be given here. Answers to the above questions can be formulated, however—at least for trial purposes. Several suggestions follow.

Earlier in this paper the overspecialization of research personnel was described. It was intimated that their education and keeping-up-with-the-literature reading absorbs a large part of their lives. If they choose to engage themselves in two fields they cannot keep fully informed in either field. Yet all these personnel are very high on the IQ scale. Our combinational habits of thought tempt us with the conclusion that scientific progress is limited by the chance production of geniuses in the human population! The successful scientist of yesterday embodied in an organic structure, a single human being, information from many scientific fields. Cortical complexity provided the synthesis of general theories; cortical complexity provided evolutionary potential in research design. The hope for the acceleration of scientific progress beyond the chance production of geniuses lies in theorism implemented by a directive social structure among scientists simulating cortical complexity. Required then, is high-order heuristic synergism among the specialists of clinical medicine, physiology, biometrics, physics, chemistry, psychology, . . . ; a synergism *motivated toward a noble achievement, communicated by fellowship, and guided by group conscience*.

After a general theory has been proposed by a community of scientists, certain intermediate techniques must be employed before the scientists may go to work in their laboratories. Logical technicians work over the postulate matrix and solve for formal inconsistencies. If found, inconsistencies are then referred to the community of scientists for psychomathematical interpretation. The postulate matrix is revamped until consistency is maintained. Then probabilists and logicians work out genealogies of formal consequences of the general theory,

which consequences are interpreted by the scientists for scientific import and experimental feasibility. Criteria of experimental proof of hypotheses are agreed upon by the whole community. These criteria make reference to hierachal priority of experimental hypotheses in the hypothesis structure corresponding to the level of organization in the formal consequences; to probabilities of errors in decisions from experiments; and to standardization of terminology. Experiments are then planned to test experimental hypotheses relevant to particular formal consequences.

Throughout this process of research design as applied to medical research, biometrics can play a balanced role (as opposed to the imbalanced role under combinationalism). Biometricians generally have background knowledge in biological and medical experimental science; they deal daily with experimental design problems in all sorts of research fields; they are applied mathematicians; and they think in terms of hypothesis construction and testing. Thus they can serve to catalyze intellectual intercourse between the intuitionists, the technicians, and the formalists. Biometrics through mathematical statistics has ready-made probability models for implementing the deduction of formal consequences of general theories, for establishing criteria of proof, and for designing experiments. Finally, biomathematics, a part of biometrics, constructs mathematical models for biological dynamics, mechanisms, and operations. Such models may serve as entries in the postulate matrix of a general theory or as bridges across gaps in experimental design wherein the experimental test of certain individual hypotheses is not feasible physically, but experimental test of a mathematically compound hypothesis is feasible. The biometrician in the community of medical scientists liberates the experimentalists, for whom he has the highest regard, from any supposed necessity for being a mathematician. Biometrics promotes the maximal utilization of the experimentalist's virtues, namely, experimental dexterity and an intuitive grasp of the nature of the biological processes under his prolonged study. Between the biometrician and the experimentalist there is respect and counterrespect, humility and counterhumility.

A closing remark concerning a philosophic postulate of theorism should be made. Theorism, like religion, is optimistic. It presumes that there are alternative courses of history open to human choice. It proposes that in the long run the "best" series of alternatives can be selected by man, best in the sense of maximizing something, such as information or scientific progress.

SCIENCE ON THE MARCH

BIGGER AND BETTER FOREST TREES FOR SWEDEN

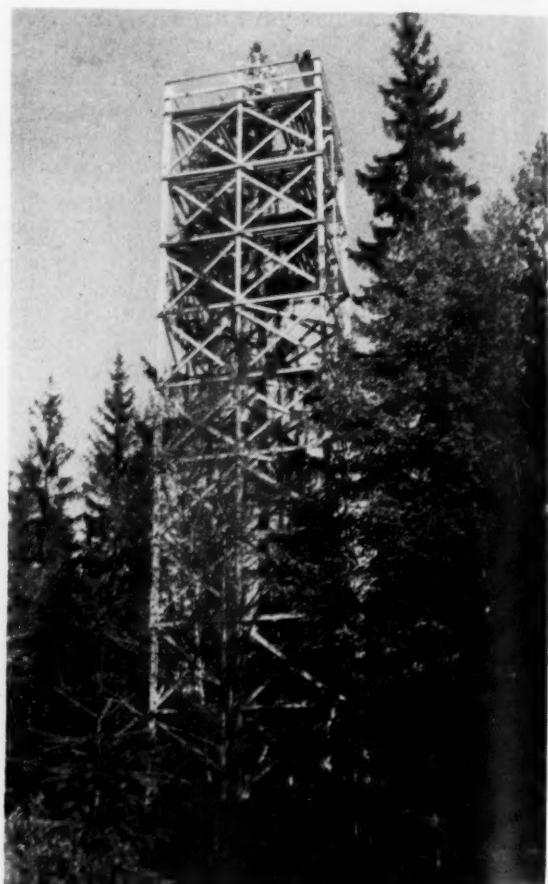
IN THE summer of 1949, I had the privilege of seeing some of the work of the Swedish Tree Breeding Institute. This Institute is a research organization dedicated to the investigation and development of hybrid and other fast-growing trees of superior quality and disease resistance. It is supported by wood-using industries, forest landowners, and the Swedish government. The project has an annual budget of 400,000 kroner, or about \$110,000 at the exchange rate prevailing in the summer of 1949. The work is carried on at three stations: at Ekebo in the hardwood region of the southwest; at Brunsberg in the conifer-hardwood belt of central Sweden; and at Sundmo, which is further north (latitude 63°) in a zone with conifers strongly predominant. The project was initiated in 1936 at Ekebo.

The interest in tree breeding in Sweden apparently had several stimuli. The great strides made in genetics in the past twenty years, especially in agronomy and horticulture, acted as an incentive. Moreover, there was widespread recognition by Sweden's foresters that in the past century there had been too much cutting of their largest and best trees, leaving the genetically less desirable individuals to reseed the cutover areas. This practice is called "high grading" by the professional silviculturists, who generally frown on the practice. Unfortunately, high grading is all too common in the cutting of most forests, much more so in the United States and Canada than in Europe.

The physical setup of the Swedish tree-breeding stations consists of a well-equipped laboratory, greenhouse, cold frames, office, a library of technical books and journals, one or more residences, and a considerable area of land for progeny tests and field trials. Near by one finds large wooden towers and scaffolds built around tall, carefully selected phenotypically elite specimens of conifers such as Scotch pine (*Pinus sylvestris*). Such a tower, from a distance, looks not unlike the frame of one of our forest fire lookout towers, with a tall, clean-stemmed tree growing up through its center. A series of wooden ladders is used to ascend the structure. Near its top, a series of scaffolds or platforms gives access to all parts of the crown of the tree for purposes of pollination. Twenty such towers have already been constructed near Brunsberg.

In the hybridization studies, glassine bags are placed over the ends of the flower-bearing branchlets, and pollen from other carefully selected trees is injected into each bag by piercing it with a pointed glass-rod syringe. Another bag is immediately placed over the punctured bag and tied near its mouth to prevent ingress of other pollen. Pollination of spruce is usually done between May 1 and 15; for pine, the period is May 23-June 7. After the cones are developed, the glassine bags are removed. Only after three crossings, and progeny tests, is a final decision made as to whether a particular specimen can be classed as an "elite" tree.

Dr. Enar Andersson, in charge of the work at Brunsberg, showed me such a scaffolded elite



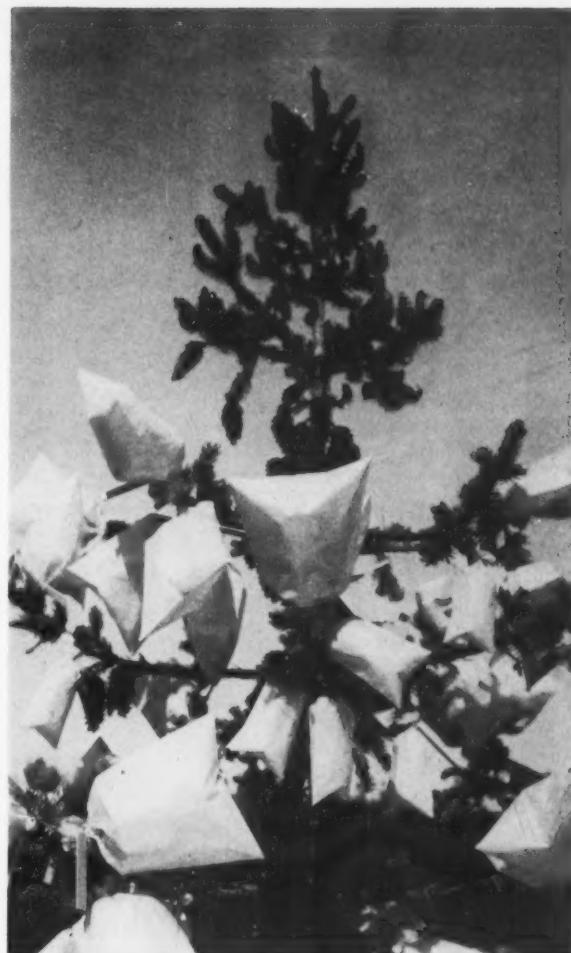
Tower built around spruce tree to facilitate access to the crown for hybridization studies.

Scotch pine. The tree was one hundred years old, 31 meters (102 feet) high, and 45 centimeters (17.7 inches) in diameter at breast height. According to Dr. Andersson, an elite tree must have a clean bole, narrow crown, thin limbs, relatively few limbs per whorl, good growth rate, and adequate resistance to disease. It must have the typically yellow-orange color of the upper bark extending well down on the main stem. The poor type of Scotch pine has the rough, dark-colored bark extending well up on the stem; this characteristic is a sign of knotty lumber in the butt log. Freedom from numerous coarse limbs is essential, because trees with thin branches prune themselves more readily.

The average cone of Scotch pine will contain some 60 viable seeds, and, since a single tree may produce 600-800 cones per year, the plant breeder may, in a decade, expect to obtain one-third to one-half million seedlings from one elite tree. In this area, Scotch pine produces some cones every year, but good seed crops in Norway spruce (*Picea excelsa*) occur only once every seven or eight years; in the other years, the yield may be one half or less. In good years, Norway spruce produces 300-350 cones, with a maximum of 600, each containing around 180 viable seeds. The production of seed from scaffolded pines and from other conifers is regarded by the geneticists as an interim measure to extend for a period of about twenty years until grafted pine specimens come into production.

As a practical immediate measure for improving somewhat the genetic quality of present seed collections, the Swedes are locating stands of good type and cutting out all the poor specimens, referred to as minus (-) variants. The remaining stands then consist of average to good individual trees, and seed collected from them will be better than from unrogued stands. To furnish the present needs of Sweden, some 56,000 kilograms, or 123,200 pounds, of conifer seed are needed per annum to provide nursery stock for the 60,000 hectares (148,200 acres) of land requiring reforestation or regeneration by artificial seeding. This will require a minimum of 5,000 acres of good "tree seed farms" for sustained production of all tree seed needs, including carry-over needed to provide seed during years of low yield. The best production of conifer seed is reported from Denmark, where as high as 50 kilograms of good seed are obtained per hectare (110 pounds per acre). In Sweden, however, average yields will probably be around 25 pounds per acre for larger tracts.

In former years much tree seed for planting in Sweden was purchased in Germany, but it was found by practical experience that such seed, par-



Flower-bearing branchlets of spruce bagged to control pollination.

ticularly that of Scotch pine, was not too well adapted to its new habitat. Today rather rigid standards prevail in regard to the place of origin of the seed and average form quality of the stand. Seed of local origin is preferred, and care is taken to avoid as much as possible the planting of Scotch pine in areas more than 100 meters higher or lower than the original habitat of the parent trees; in the case of Norway spruce the allowable limit is 200-300 meters.

Another technique used in the tree-breeding work is the grafting of conifers. Elite conifers are propagated by grafting their scions on potted seedlings, usually two or three years old. The potted plants are placed in the greenhouse at the end of January for two weeks, using 500-watt lamps to hasten growth. The grafting operation is performed in February. Pine grafts are about 90 per cent successful, in spruce only about 60 per cent. After one or two years, the grafted conifers are planted in



Tallest tree, at left of center, is a genetically elite Scotch pine. It has a long, clean bole, a narrow crown, and fine branches, and is in considerable contrast with tree at extreme right, which represents a more nearly average form.

a "seed tree orchard" at a spacing of at least five by five meters (16.4 feet). When about waist-high, they may be cut back, and 6-8 inches of the top and some of the tips of the side branches removed, to form a lower, denser-crowned tree. Such plants are easier to care for, and future seed collection is thereby simplified. Aftercare involves cultivation, weeding, pruning, application of fertilizers, and any other measures required to get the seed tree farm into actual production as quickly as possible. In the case of Scotch pine, this will be at about fifteen to twenty years of age. Tree seed farms will be kept away from other trees of the same species for a distance of one to two kilometers (0.6-1.2 miles) to minimize chances of pollination of the elite stand by rogues.

At present, greenhouse grafts can be made at the rate of 100-150 per man-day. The final total cost per graft when ready to field plant is about 5

kroner, or \$1.39 each at the rate of exchange prevailing in 1949. Once the project is out of the pilot-plant stage, production costs can no doubt be reduced. Just recently, a grafting technique was devised which is suitable for use in the nursery. By avoiding the greenhouse expense, costs have been reduced to one-half to one kroner (14¢-28¢) per plant.

Before final selection of a tree for propagation by grafting, small-scale progeny tests are run in the field. These usually consist of four replications of 50 trees planted in rows or small rectangular blocks, with trees spaced at 1.2 meters (3.9 feet). Blocks are preferred rather than rows because of occasional sharp suppression or shading in case an exceptionally vigorous rapid grower happens to be alongside a slower-growing variety or strain. The suppression problem in progeny tests is more acute in broadleaf than in coniferous species. Trees in progeny tests are given mechanical cultivation and hand weeding, and the sites are selected for maximum uniformity to provide comparable conditions.

At Brunsberg, about 15 hectares, or 37.0 acres, of progeny tests are planted every year, and there are two nurseries of about 2 hectares each where elite stock is propagated and various other tests can be run. Field observations on progeny tests include survival counts, measurement of total height and diameter at one-meter intervals for volume computation, and comprehensive notes on form, vigor, limb character, and resistance to insects, disease, and snow breakage.

The breeding of broadleaf species offers maximum possibilities for large-scale production of elite stock by crossing of various species through controlled pollination. Very successful among these, to date, have been crosses of Swedish and North American species. They were so encouraging in initial tests made about seven years ago, that Dr. Helge Johnsson, in charge of broadleaf work at the Tree Breeding Institute at Ekebo made a special trip to the United States and Canada in the fall of 1947 to select elite specimens of aspen and paper birch, from which twigs were collected in December by personnel of the U. S. Forest Service, Dominion Forest Service of Canada, and other foresters. These were sent to Sweden by way of the Harvard Forest at Petersham, Massachusetts, where the shipments from various areas were assembled, and where some of the material was retained for genetic studies by Dr. Scott Pauley and associates of the Cabot Foundation. This project can definitely be classed as a milestone in international cooperation in forest research. Promising crosses performed by Dr. Johnsson involve species of Euro-

pean and American aspen (*P. tremula* \times *P. tremuloides*), alder (*A. glutinosa* \times *A. rubra*) and birch (*B. japonica* \times *B. verrucosa*).

Progenies of the best interspecific crosses of aspen from the two continents produced 33.0 cubic meters of wood per hectare, or 472 cubic feet per acre, in seven years, whereas progenies of the best of the intraspecific crosses involving selected native clones of *P. tremula* yielded only 7.3 cubic meters (104 cubic feet per acre) in the same period, thus representing roughly a fourfold increase in growth in the intercontinental hybrids, compared with native Swedish hybrids.



Tying a pine graft.

Progenies of somewhat later crossings of *P. tremula* \times *P. tremuloides* made in the spring of 1945, and planted as 1-0 stock, had average heights of 20 feet and diameters of 2.5 inches in five years, whereas progenies of crosses involving only *P. tremula* had only half the height and only one inch of diameter at breast height. It is a striking sight to see the contrast in growth between the vigorous intercontinental hybrids in rows next to the ordinary hybrids in some of the progeny tests at Ekebo. An interesting side light is the fact that the aspen hybrids are quite resistant to the leaf disease *Epidendrum* sp.

Alder crosses (*A. glutinosa* \times *A. rubra*) have produced trees about 20-25 feet high and 3 inches in diameter in six to eight years. Alder will grow to a merchantable-size tree in Sweden and is of silvicultural interest because it is adapted to wet sites, which it gradually dries up and improves, not only physically but also chemically, because of its capacity to fix atmospheric nitrogen in its root nodules.

Crown and bole form and freedom from disease of the broadleaf species were of particular interest to the Swedish forest genetics experts; straightness of stem was also of paramount importance in the

selections. Thin limbs and rather horizontal branching were other qualities desired, especially in aspen.

Dr. Johnsson stated that there is good evidence that much of the crookedness and forking in beech, and possibly in other hardwoods, was inherited, and he inferred that these are factors which the geneticist could, to a large extent, control in tree breeding.

Aspen crosses may be made in the greenhouse by the simple method of placing flower-bearing twigs of the desired individuals in jars of water on the greenhouse bench and forcing at will. If it is desired to preserve the parental trees for future crosses, twigs may be bottle-grafted on seedling stock. Such crosses are usually made in February or March in a sealed-off chamber free from other pollen. Many thousands of viable seeds can be produced from a single crossing, thus providing opportunity for rapid expansion in production of certain



Eight-year-old hybrids of aspen. Row at right center has tall trees which are intercontinental crosses of northern European and American forms of aspen (*P. tremula* \times *P. tremuloides*). Rows of shorter trees in center and at left are crosses of the northern European form of aspen (*P. tremula* \times *P. tremula*).



Bottle graft of aspen.

selections to furnish large numbers of seedlings for mass planting. Bottle grafts are successful in about 90 per cent of the cases with aspen.

Birches are grafted in March. The 1-1 stock is preferred for progeny tests although 1-0 stock is often quite large enough for field planting. At ten years of age, considerable seed production can be obtained from birch. A single large birch can produce as many as two to three million seeds per tree, so in theory a very small acreage of "tree seed farm" could produce all Sweden's needs for birch.

The whole program of the Swedish Tree Breeding Institute is indeed impressive. It seems boldly and imaginatively planned, and reflects great credit on the vision of workers in the forest industry and government, and of forest landowners in undertaking such a long-range and epochal undertaking. In my opinion, it rates as the most outstanding new forest research project I saw anywhere in nine western European countries in 1949.

JOSEPH H. STOECKELER

*U. S. Forest Service
Rhineland, Wis.*

Figures 1-4, 6, courtesy of Swedish Tree Breeding Institute.

SCIENTIFIC RESEARCH IN BRITAIN'S COLONIES

IN THE eyes of history very few years have passed since the pioneers who went out into the wilder places of the earth did so in ox wagons and fought the wilderness with ax and hoe. "The pace of Africa," said one of them who is still alive today, "is the pace of the ox, and she cannot go any faster." Now you can have lunch in London today and breakfast in Nigeria tomorrow, and bulldozers and tractors are opening up as much land for cultivation in a week as that old pioneer cleared in a whole year's work.

The development plans of Britain's colonies are ten-year plans, and in that short time they will achieve more than the accomplishment of all the centuries already past. Speed, however, is not the only revolution which science has brought to the pioneering of today. If it were, the great adventure would be doomed before it started, for nature would retaliate with all its ancient armor. If disease could not be overcome, man in the tropics, white or black, could achieve no more in the future than he has in the past, and, if tropical soils were ignorantly exploited, the tropics would, within a lifetime, become a wilderness.

The essential basis of all practical achievement

is now recognized to be scientific research, and this is just as necessary in the economic and sociological fields as it is in those of medicine or engineering. The tropics are cruel, and they do not tolerate mistakes; many a promising enterprise in Africa and elsewhere has had to be abandoned, and many stout hearts have been broken, because endeavor was not based on the knowledge that only the scientist can give.

When, therefore, the government of the United Kingdom, in partnership with the colonial peoples, embarked on its great policy of colonial development, it remembered these misfortunes of the past and not only allocated large sums for research of all kinds but set up a committee of distinguished scientists, which subsequently expanded into the Colonial Research Council, to advise on the use of the money.

By March 1949, nearly £6,000,000 had been spent on research in the colonies, and the rate of expenditure is rapidly increasing. Altogether, more than three hundred research schemes have so far been approved. They include, for instance, the establishment of research centers designed to cater to almost every kind of human activity—medicine,

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Members of the research station at Tafo in the Gold Coast Colony in British West Africa placing insects inside muslin cages on shoots of the cocoa tree. The station is staffed by fourteen Europeans and twenty-nine Africans engaged in fighting the parasites and pests that spread disease on the cocoa farms.



Dr. V. van Someren, resident biologist, at work in his laboratory in the River Research and Development Center on the upper Sagana River in Kenya. Dr. Someren is studying the reaction of a trout to the local river water and its underwater food.



Removing the brain from a dead sheep, Medical Research Institute, Yaba, Nigeria, British West Africa.

anthropology, agriculture, veterinary science, forestry, fisheries, locusts, and buildings—and they are, for the most part, being organized on a regional basis to serve the particular needs of East or West Africa, the Eastern territories, or the West Indies, as the case may be.

The work of Britain's Colonial Research Council is by no means limited to the activities of colonial research centers; it also facilitates, supervises, and coordinates hundreds of individual research projects by scientists, or groups of scientists, working in the field. Their studies range from linguistics, ethnography, and malnutrition to native marriage customs and systems of land tenure; from the problems of crop storage in East Africa and the swollen-shoot disease in cocoa in West Africa to the economic possibilities of ceramics in Barbados.

Some of the results of this work may not, for the layman, be spectacular, and some will take years to appear, but research, though not on the present scale, has been carried on in the colonies for many years, and already some dramatic discoveries have been made. In medicine alone, the discovery of specific preventives against malaria, yellow fever, and the typhoid group of fevers is of even greater importance, perhaps, than the cures that have been found for sleeping sickness, yaws, and other tropical diseases.

The use of the new insecticide DDT appears to have wiped out malarial mosquitoes and other harmful insects in all the most thickly inhabited parts of British Guiana, which is an achievement of immense significance for the whole of the trop-

ical world. The new drug antrycide may prove to be such a powerful defense against trypanosomiasis—though this is not yet certain—that vast tracts of Africa at present closed by the tsetse fly may be opened up for cattle-breeding and the resettlement of surplus population from overcrowded areas.

These are only a few examples of the incalculable practical value of scientific research, and it is not always the dramatic discovery that is the most valuable. In agriculture, particularly, results are slow, but it is on the ever-growing sum of knowledge about tropical soils and their products that increased and lasting productivity depends. Soil surveys are, for instance, now regarded as a prerequisite for all major schemes of agricultural development. Indeed, the whole technique of survey is being more and more widely used in the early stages of every kind of development.

At one end of the scale a social anthropologist makes a detailed survey of the economics and habits of the people of a town before a slum-clearance scheme is started; at the other there is the central Directorate of Colonial Surveys which, in cooperation with Britain's Royal Air Force, has been set the task of carrying out an aerial survey of the whole colonial empire and has already photographed areas in Africa totaling four times the size of the United Kingdom. As in war, so in this vast enterprise for peace, it is on science that everything else to a great extent depends.

KENNETH BRADLEY

*Colonial Office
London, England*



THE SCIENCE REPORTER

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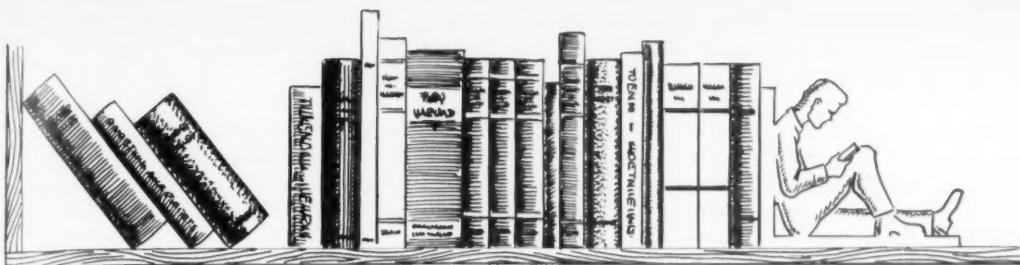
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BOOK REVIEWS

THE PROPER STUDY OF MANKIND

Francis Bacon: Philosopher of Industrial Science. Benjamin Farrington. xvi + 202 pp. Illus. \$3.50. Henry Schuman. New York.

Goethe as a Scientist. Rudolf Magnus. (Translated by Heinz Norden.) xxii + 259 pp. \$3.50. Henry Schuman. New York.

BECAUSE they were two of Europe's greatest minds, Bacon and Goethe have inspired a voluminous literature. It is not inappropriate to consider them together, for they had some things in common. Each was a man of letters as well as a philosopher of science. Each was a genius in sheer amount of accomplishment. Each left an imprint on the world. Both were endowed with prodigious imaginations. This pair of books, the eleventh and twelfth numbers in the "Life of Science Library," helps round out the portraits of these heroes.

In dealing anew with Francis Bacon, Professor Farrington, of University College, Swansea, omits all but the most pertinent biographical details and expounds his Lordship's ideas against only a sleazy backdrop of Elizabethan England. His story is of a man with whom the pursuit of philosophy was a sublime hobby, which he rode hard, devoting to it all the time and energy left over from his duties as Lord Chancellor of England. Typical of his ardor is the ironic manner in which he met his death. It is related that while on the way to Highgate Bacon stopped one cold March day to perform an experiment that, if all had gone well, might have given the world deep-freeze 325 years ago. (And how that would have gratified him!) Curious as to the effect of cold on the preservation of meat, he halted on his journey, purchased a hen, killed it, and stuffed it with snow, but, alas, in thus exposing himself he caught a chill and died of bronchitis a few days later.

Bacon died without giving the world a single practical invention or scientific discovery, but he must be classed as one of the greatest of creative scientists. He threw overboard, as hopelessly stagnant, the philosophers of antiquity, particularly Aristotle, and with a Gargantuan ambition set out to offer the world a new blueprint for the future and a glowing picture of a utopia run by scientists and inventors. The goal of science, he said, is to endow human life with new discoveries and powers. This was a new idea, and by championing it he became

the great protagonist of industrial science. He tried to lead men toward the creation of new arts that would lift the human race out of its misery. He complained of the "premature despair in human enterprises." The cure for this despair, he said, is to find some way to make natural philosophy operative to relieve the inconveniences of man's estate. He would be surprised, we imagine, if he could return today, to see how many of man's inconveniences have been relieved through a multiplicity of inventions, but to see too that these have not dispelled man's despair nor seem to have any prospect of doing so.

There were, as Professor Farrington points out, many gaps in Bacon's reasoning. He has not been swallowed *in toto*. But he sowed the seeds of an intellectual revolution whose harvest is not yet past. The beauty of Farrington's book is the clear manner in which he has presented for the general reader Bacon's ideas and explained the meaning of his works, especially *The Great Instauration*, which includes the *Novum Organum*. How the Verulamian philosophy broke with the "wisdom of the ancients" and what Bacon achieved by the publication of his own theories are clearly set forth. "Bacon's interest went beyond a reform of logic and was aimed at the transformation of the material conditions of life." His New Logic was only a part of his magnum opus.

Like Bacon, Goethe had too many irons in the fire, but being a creative genius he pulled most of them out successfully. *Goethe as a Scientist* is a translation and reissue of the late Professor Rudolf Magnus' *Goethe als Naturforscher*, first published in Leipzig in 1906. It is a revealing book and strengthens what we already know—that Goethe belongs in the class with Leonardo as one of the intellectual titans of all time. His literary powers so overtake any other aspect of the man that we often forget that he more than dabbled in science. Romantic though he was in his view of nature, he was also an earnest experimenter in a variety of fields of scientific investigation. In botany he proposed the "archetype" concept and formulated a theory of plant metamorphosis (*Versuch die Metamorphose der Pflanzen zu erklären*, 1790); in osteology and comparative anatomy he demonstrated the presence of the intermaxillary bone in the human skull, in common with other mammals; he

developed theories in physiological optics and tried to prove Newton wrong in the concept of color (*Beiträge zur Optik*, 1791-92); he studied mineralogy, geology, and meteorology (the mineral goethite was named for him). Many of his concepts in biology foreshadowed Darwin.

All these contributions and researches Professor Magnus describes in interesting detail. If Goethe also discovered some things that aren't true—well, so have many other investigators. But right or wrong, Goethe did not withdraw into the laboratory; he entered with characteristic gusto into current scientific controversies and was a moving and influential spirit in scientific as well as literary thought. The marvel is that in this man in whom the creative imagination was so dominant there was also the irresistible urge toward the search for truth after the scientist's objective manner. In the end science was bound to benefit by this dual approach. As Helmholtz said, "Every scientist who seeks to arrive at really broad views needs something of the poet's imagination." Though the fame of Johann Wolfgang von Goethe has not rested upon his science, nevertheless science was a part of his genius, and Magnus' book contributes definitely to our understanding of a most complex human being.

PAUL H. OEHSER

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Charles Darwin. The Naturalist as a Cultural Force.

Paul B. Sears. ix + 124 pp. \$2.00. Scribners. New York.

THIS is not a book about Darwin and his works so much as it is a brief discussion of his influence on the thought of our times. It is one of the first volumes in a new series whose purpose "is to give the intelligent layman a basic understanding of those thinkers of the last hundred years who have most influenced the intellectual currents of our time." It is safe to say that Darwin and his thought is one of the most difficult assignments in this project, if for no other reason than that there is already a vast library on this theme. Yet we must not forget that there are always new and eager readers for such books as this.

In such a short book it is surprising to find so many aspects of modern thought discussed. Religion, from Bishop Wilberforce to Scopes, of course; the mutationists, orthogeneticists, and neo-Darwinians all receive their notice, and the pragmatists and modern education occupy an entire chapter. As we might expect from Dr. Sears, the influence of Darwin's work on ecology and the importance of this branch of science to man are well brought out and adequately discussed. The author's position on many controversial points will be plain to biologists, but the wrong impression may be gained on such matters as orthogenesis by those for whom the book is specifically intended, since they often try to read even faster than they run. Nevertheless, as an introduction to the history and great issues of Darwinian thought, this little book is a lucid and successful contribution. Biologists, however, will miss any consideration of Darwin's

influence on taxonomy, or mention of his masterly accomplishments as a systematist.

As an introduction to the subject, the book is provided with a rather skimpy bibliography, which lists only the *Beagle Journal*, of Darwin's many works, and fourteen standard biographies and general books on Darwinism. To be sure, most of these books have bibliographies of their own, but nowhere in this book will be found the complete title of the *Origin of Species*.

JOEL W. HEDGPETH

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The Life of William T. Davis. Mabel Abbott. xv + 321 pp. Illus. \$3.50. Cornell University Press. Ithaca, N. Y.

THE biography of a quiet and friendly entomologist, Billy Davis of Staten Island, is introduced by the history of his mother's family—which is almost the history of Staten Island itself. His father, of New England stock, came to Staten Island when it was having one of its periodic booms, and settled there.

From early youth, Davis had a curiosity in regard to nature, and a keen power of observation. Little is known of his early training except that during the several jobs he had as a youth, all his spare time was spent roaming the countryside. When he was fifteen, there emerged hordes of the "seventeen-year locust," last there in numbers two years before he was born. He was to observe this phenomenon repeated seventeen years later. His first *Natural History Notes*, written in 1879 when he was seventeen years old, shows considerable knowledge of insects and their classification.

He was one of the founders of the Natural Science Association of Staten Island, with colleagues who were prominent naturalists in the vicinity. In 1882, on his first extensive trip, with Professor N. L. Britten, he made a large collection of Colorado potato beetles, for the purpose of studying the variations occurring among them.

He worked for an insurance firm and lived in two worlds—his daytime life was figures, successful because of his methodical aptitude; out of the office, he was in the field again, collecting and studying. Never in robust health, he was often away from his office, but when there he kept his eyes open, once discovering some rare beetles in the Produce Exchange, where they were living in food samples.

He was an avid reader (when he did not see "specks and blotches" before his eyes, due to a bad liver), not only of scientific journals but of good contemporary history and fiction, and was interested in local history and genealogy, many details of which he gathered from old graveyards of the island.

Despite his broad general interest in natural history, he specialized in his first love, the cicadas, and became an authority on this group of insects, their classification, and habits. The list of published works at the back of

the book shows that each year between 1883 and 1925 he produced a steady series of papers on animal life; on beetles, crickets, frogs, mice, bumblebees, and oysters.

WILLIAM M. MANN

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Albert Einstein, Philosopher and Scientist. Paul Arthur Schilpp, Ed. xvi + 781 pp. \$8.50. Library of Living Philosophers. Evanston, Ill.

THE reputation of Einstein rests, in popular opinion, upon his theory of relativity; but this is by no means his only scientific work. His contributions to quantum theory have been such that had he written nothing on relativity his standing as a theoretical physicist would still have been equal to that of Planck; and he has also made an important contribution to the explanation of Brownian motion.

The book under review shows that among philosophers Einstein's standing is in no degree inferior to his reputation among physicists. It begins with an "intellectual" autobiography in which Einstein traces the growth of his interest in science from boyhood up to recent times. He tells how, at the age of twelve, a little textbook of geometry came into his hands at the beginning of a school year. An uncle had told him of the Pythagorean theorem before he had seen this book. After much effort he succeeded in proving this theorem on the basis of similar triangles. He follows up the development of his scientific interest through his years at the Polytechnic Institute of Zürich, where he came under the influence of Minkowski. He also describes the later difficulties with which he had to deal in passing from the special to the general theory of relativity, which required eleven years for their overcoming. He closes the autobiography by saying: "This exposition has fulfilled its purpose if it shows the reader how the efforts of a life hang together and why they have led to expectations of a definite form."

There follows next in the book a series of twenty-five "descriptive and critical essays" on the work of Einstein, contributed by philosophers and scientists from eleven countries, among whom are six Nobel prize winners. The subjects of these essays range from "The Scientific Work of Albert Einstein" to "Einstein's Influence on Contemporary Philosophy."

The book next gives Einstein's reply to these essays, in which he says that in spite of serious efforts he has been unable to quite understand some of these essays—in which difficulty this reviewer sympathizes with him.

The book closes with a bibliography of the writings of Einstein to October 1949. The compiler says of this that it contains a fairly complete list of his scientific work, but that the published material representing his other interests is much more difficult to garner, and the record of it is clearly inadequate. However, the bibliography as given is an excellent and valuable source of reference.

There is one important point in connection with the theory of relativity which is not mentioned in this book.

The special theory of relativity arrived at the conclusion that it is impossible by any experiment that can be devised to detect our absolute motion in space. Physically, this appears to be true for motion of translation, but rotational motion is a different matter. Were the earth enveloped in perpetual cloud we could still, by the Foucault pendulum or by the gyroscope, detect and measure its rotation. For this argument the theory of relativity has an answer of a kind; but, as Eddington says (*Space, Time and Gravitation*, p. 153), when we come to rotational motion relativity stops explaining phenomena and begins explaining them away.

The answer of the relativist in this case harks back to the Ptolemaic astronomy. Where we are accustomed to consider a rotating earth, accompanied by a field of centrifugal force, surrounded by an approximately stationary universe of stars, the relativist considers a stationary earth inside a hollow rotating mass located somewhere or other. Quite a step backward to the dark ages? Well, it might be if his equations did not show that a hollow rotating body will apparently exert a very small force on a body within it. A pendulum vibrating within a massive, hollow, rotating cylinder will, according to the Einstein equations, be deflected in the direction of the rotation, dragged around slightly by the rotating mass; and the equations even indicate something simulating centrifugal force inside a hollow, rotating mass (*The Meaning of Relativity*, pp. 110, *et seq.*).

Mathematically, the theory thus has an answer; but this answer, though mathematically perfect, lacks one very important property which Eddington calls "convergence." To produce this quasi-centrifugal effect, which is very small, an enormous rotating mass is required. Einstein himself says that this effect is so small that it would be impossible to detect it by laboratory experiment; and the farther off we place the reckoning the more we have to pay. The universe on this hypothesis is like a pyramid standing on its apex. Here the theory of relativity shows plainly its similarity to all previous scientific theories: useful where it fits the facts, useless where it does not.

Newton's theory of gravitation held the stage for nearly two centuries until the discovery of the anomaly in the motion of Mercury. Einstein improved Newton's theory and explained the motion of Mercury, but ran into difficulty with centrifugal force, which Newton explained perfectly. Einstein's theory is not the last word on the subject; but how long it will be before the advent of the next improved theory is a matter at present on the knees of the gods.

PAUL R. HEYL

Washington, D. C.

Allen H. Godbey, A Biography. Clarence H. Brannon. 470 pp. \$5.00. Christopher Publishing House. Boston.

THIS entertaining biography of Allen Godbey should prove to be of great interest to all individuals who are interested in men of unusual intellect and character. Dr. Godbey's facility for acquiring

languages was almost unbelievable; on the analytic side his ability was comparable to that of the late Sir Richard Francis Burton, although, needless to say, his philosophical and religious outlook was quite different from that of Burton. This remarkable linguistic ability of Godbey showed up early, as did many other facets of his intellectual power. He had read the entire Old Testament in Greek (Septuagint) before he was thirteen. Before he was of college age he had read more Latin than was offered in the curriculum of any American college or university. In later years about eighteen more languages were mastered with speed and apparent ease. His special field of interest, in which he was a recognized international authority, was the pre-Semitic Red Berber Culture and the Minoan, Philistine, and Aegean influences in South Palestine.

Dr. Godbey was born in 1864 in Missouri; he died at the age of eighty-four in Boston. The period of his life is of great interest from the standpoint of American history. The author did such a fine piece of work in discussing not only the events of Godbey's life, but also the period, that I felt on several occasions while reading the book that the title should have been "Allen H. Godbey and His Times." As the book is copiously documented, it is difficult to do it justice in a short review. But from the first chapter, A Child Prodigy, to the last, Closing Incidents, the author, who is state entomologist of North Carolina, has given us an interesting presentation of the life of a man who could have attained eminence in almost any branch of learning.

M. W. WING

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Biologie der Goethezeit. Adolf Meyer-Abich, Ed. 302 pp.
Illus. \$5.50. Hippocrates-Verlag Marquardt. Stuttgart;
Chronica Botanica. Waltham, Mass.

THIS book is an anthology half of which consists of excerpts from Goethe's morphological writings and the other half of selections in the fields of biology and philosophy of science from the works of six contemporaries or near-contemporaries of Goethe, who were influenced by his thought and who to some extent used his language. These men are Georg Forster, Alexander von Humboldt, Lorenz Oken, Carl Gustav Carus, Karl Ernst von Baer, and Johannes Müller.

The editor has contributed a historical introduction presenting Goethe's scientific methodology in the light of the previous history of the subject, a conclusion dealing with Goethe's scientific significance today, a foreword, and special biographical prefaces.

In the Introduction science is characterized as throughout its history having used one or the other of two methods: that of the ancient Greeks, or that of the Renaissance. The Greeks, on the whole, thought in static and qualitative terms. Their mathematics, culminating in Euclid's geometry, is an example of their static method. So also is their language, which was incapable of expressing action without freezing it into a rigid pattern in so doing. As for their preoccupation

with the qualitative, this factor reached its ultimate development in the magnificent hierarchy of Aristotelian forms, capped by God. Related to this phase was their love of completeness, their abhorrence of endless series and their teleology.

The scientists of the Renaissance, on the other hand, thought in dynamic and quantitative terms. For them motion and change were real. They translated qualities into quantities, thus making them pliable to mathematical treatment; and their mathematics was not the static geometry of Euclid, but the infinitesimal calculus of Leibnitz and Newton, an instrument capable of analysis of the dynamic. They espoused the idea that incompleteness and endless series are characteristic of nature, and they substituted mechanical causality for teleology.

Has the method of the Renaissance won? Meyer-Abich says No. This brings us to his Conclusion, where there is much informative analysis of Smut's holism, Driesch's vitalism, Köhler's *Gestalt*, soul, organic wholes in contrast to aggregates of parts, and the editor's personal doctrine; in short, of ancient modes of thought in modern, post-Darwinian biology and philosophy of science. This antimechanistic revival touches even physics, at the quantum level. Eventually, physics may be absorbed into biology, when the laws of mechanics will appear as limiting cases ("simplifications," to use the editor's term) of the laws of organic wholes.

The purpose of Meyer-Abich's historical analysis is to make plain where Goethe the scientist fits in. Goethe was a late champion of Aristotle and of the Greek method in general, and an early champion of today's holism and vitalism. His dislike of mathematics, and his opinion that even in physical theory mathematics is to be excluded from the reasoning process, are attributable to his Aristotelianism rather than to his poetic approach to nature. Instead of converting qualities into quantities, he deduced qualitative phenomena (his series of "metamorphoses") from qualitative primeval ("Ur-") phenomena. This was his method both in biology, where he developed a doctrine of homologues, and in physics. His theory of color becomes understandable when seen from this point of view. Meyer-Abich remarks that the belief, stemming from Haeckel, that Goethe anticipated the Darwinian theory of descent is based upon a misinterpretation. The pattern of the Ur-phenomenon and the series of metamorphoses is not that of the genealogical tree, but that of the Platonic idea or the Aristotelian form and phenomena. No Ur-phenomenon, distinct from its metamorphoses, ever actually existed as an individual in space and time. Goethe's important scientific contribution was his theory of compensation, which is alive today and has a brilliant future.

RUFUS SUTER

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Pavlov, A Biography. B. P. Babkin. xiii + 365 pp. \$6.00.
University of Chicago Press.

PAVLOV was one of the great figures among physiologists during the last half-century, and was perhaps equally well known to psychologists because of

their interest in conditioned reflexes. He lived a full life, beginning his scientific work early, and continuing actively until the end. He lived to the age of eighty-six (1849-1936). The author of the present biography modestly disclaims any desire that this should be the definitive one. Whatever later biographies may appear, this is a good one, giving a vivid picture of Pavlov as man and as scientist, and placing his scientific contributions in perspective.

Dr. Babkin, the author, is Pavlov's senior surviving pupil. He writes out of thirty-five years' acquaintance with Pavlov, during ten of which he was Pavlov's assistant. He has been away from Russia during recent years, writing from Montreal, so that the book is written without political pressure.

The volume is about equally divided between the biographical portion and that concerned with Pavlov's scientific work. The 173 pages of biography are enriched with many personal anecdotes, and by letters from Pavlov and Mrs. Pavlov to the author. The Soviet regime was kind to Pavlov, and for many years he criticized it freely. This period lasted for the years 1917-33. His utterances were much more friendly to the Bolsheviks during the last years of his life, 1933-36. The author

writes quite objectively about these matters, although his sympathies are not with the Soviets.

The remaining sections of the book concern Pavlov's scientific work: the early physiological studies of the heart and circulation, the work on the digestive glands (which won Pavlov the Nobel Prize in 1904), and finally the work on conditioned reflexes. This last phase of his scientific endeavors occupied the thirty-four years from 1902 until his death in 1936.

The section on conditioned reflexes is enhanced by placing Pavlov's work in the historical context of Sechenov and Hughlings Jackson. These two men are said to have set the stage for the unification of physiology and psychology. Pavlov then moved on to the study of the functions of the cerebral hemispheres from a strictly physiological point of view.

Pavlov combined the best in the romantic tradition of science with that of the classical tradition. He exuded warmth and enthusiasm, but insisted on the patient repetition of detailed observations. The reader comes away liking Pavlov as well as respecting him.

ERNEST R. HILGARD

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PRIMITIVE ARTISTS

A Short History of Chinese Art. Hugo Munsterberg. xiv + 227 pp. Illus. \$5.00. Philosophical Library. New York.

PROFESSOR MUNSTERBERG'S book is a very readable and useful piece of work. He traces the development of Chinese art from its very beginnings down to the early part of the last pre-Revolution dynasty, that of the Manchus. He begins with a brief discussion of the origins of Chinese civilization and certain forms of already known prehistoric art of China. This is followed by the Shang and the Early Chou Period, the Han Period, the Six Dynasties Period, the Sui and T'ang Periods, the Sung Period, and Yuan, Ming, and Ch'ing Periods. Horizontally, the range of subjects treated is equally extensive. It includes pottery, bronzes and jades, painting, architecture, sculpture, and ceramics.

On each of these periods and subjects Dr. Munsterberg speaks with understanding and authority. The narrative, begun in each chapter with sketches of history to indicate their bearing on art, is simple and highly interesting. The bibliography has been selected with taste and excellent sense. The fifty plates constitute one of the best presentations of Chinese jades, bronzes, paintings, and porcelain in one small volume that the reviewer has ever seen. The reproductions are very good.

Occasionally the reviewer has found some passages on which he would disagree with the author, or at least has found the statements given debatable. For example, at one point the author says: "It was formerly believed that the various designs found on the Prehistoric pottery, not only of China but also throughout the Near East, were purely ornamental in character . . . more recently, the opposite point of view has been held, for it is now thought that every design must necessarily be

magical and symbolic in nature, and that purely ornamental designs did not exist" (p. 6). I do not know Dr. Munsterberg's sources for the latter part of this statement, but it is my strong impression that modern archaeologists and anthropologists who have to interpret prehistoric data will not take either view merely as a matter of dogmatic belief, without reference to total context in which each particular group of data had been uncovered.

A few debatable points notwithstanding, Munsterberg must be congratulated on being able to present so much and so well within so little space. The book is a "must" in any reading list on Chinese culture or art, either for the student or the lay reader.

FRANCIS L. K. HSU

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Native Arts of the Pacific Northwest. Robert Tyler Davis. xi + 165 pp. 200 illustrations. \$7.50. Stanford University Press.

WE welcome this very handsome volume as the first in the "Stanford Art Series," edited by Edward M. Farmer. It deals with examples of Northwest Coast Indian and Alaskan Eskimo art, collected by Axel Rasmussen, chiefly during the years when he was superintendent of schools in Skagway, Alaska. It had been Rasmussen's intention that his collections should be housed in a museum in Skagway, but he died before this purpose could be accomplished. The materials became scattered after his death, and it is due to the energy and foresight of Earl Stendahl that they have been reassembled in the Portland (Oregon) Art Museum.

The two hundred specimens here illustrated have been selected from the five thousand objects in the collection. Since a large proportion came from the northern Tlingit Indians, among whom Rasmussen lived for so long, and since no other specimens than his are included in the book, it should not be assumed to illustrate the entire range of Northwest Coast art. As far as the reviewer knows, however, none of the material has previously been published, and some pieces are of unusual artistic or ethnographic interest. Certain types of material, such as houses, canoes, graves, and totem poles, which are difficult to collect, are omitted or but slightly represented in this book. The Eskimo material is more obviously limited than is the Indian, and might perhaps have been omitted to give more space to the latter.

The introductory text, which serves to present the material in its social and cultural setting, is written by Robert Tyler Davis, formerly director of the Portland

Art Museum, and now director of the Museum of Fine Arts in Montreal and a professor at McGill University. There is also a catalogue of all specimens illustrated, based on Rasmussen's notes, which is very valuable. The photography, by William Reagh, and the layout and typography, by Alvin Lustig, are magnificent.

In these beautiful and expertly presented illustrations the objects lose little of their three-dimensional qualities. We can almost feel the surface texture, the swelling curves, and solid mass. We feel also the impact of that dramatic vitality which makes Northwest Coast art the most vigorous of native American artistic expressions. This is a volume to be treasured by both artists and anthropologists.

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OUR MERCHANTMEN

Merchant Ships (1949-50). E. C. Talbot-Booth, Ed.
260 pp. Illus. \$8.50. McGraw-Hill. New York.

THE end of the recent war left the world with but a small percentage of the old familiar names and types of ships. We loved those ships and deeply regret their passing, although we must admire the new, sleek, powerful beauties now coming off the shipways. Thus *Merchant Ships* will interest all those who follow the sea, who are active in one of the many shipping ventures, or who are, perhaps, just armchair adventurers. In a very comprehensive manner the book gives instruction in ship recognition, names systems and comparative sizes. A large section entitled "Recognition Drawings," with nearly two thousand accurately drawn silhouettes of active ships, will doubtless intrigue many readers.

There are pages of fine clear photographs of some four hundred and fifty present-day vessels, complete with the principal facts of size and power. From photographs one delves into pages illustrating "Company Flags and Funnels," "Ship Types," and particulars of "Ships Building and Completed." The "War Loss Section" lists some six thousand Allied ships, and in those packed pages are many old friends. The final index section adds a quick reference to the three thousand or more live ships illustrated by silhouette or photograph within the book.

The compiler of this volume has turned out primarily a textbook on merchant ship identification. Type, size, speed, etc., of named vessels are accurately given. What a wonderfully useful textbook for our armed forces in training men and officers quickly to identify a specific vessel—and what a fine ready-made reference in the hands of a potential enemy, who perhaps, though not a great maritime nation, would in the event of hostilities find accurate identification of merchant vessels as observed in the horizon-sweeping eye of a periscope most valuable!

CAPTAIN W. R. LAWRENCE

New York City

THOUGHTS, OBSERVATIONS, AND CONCLUSIONS

Phenomena, Atoms and Molecules. Irving Langmuir, xi + 436 pp. Illus. \$10.00. Philosophical Library. New York.

THIS book is a collection of articles selected from over two hundred papers published by the celebrated American physical chemist Irving Langmuir (Nobel Prize in chemistry in 1932) since he joined the research group in Dr. Whitney's Laboratory of the General Electric Company in A.D. 1909. The first chapter of the book, *Science, Common Sense and Decency*, represents the retiring presidential address of the AAAS in 1943. The second chapter covers Dr. Langmuir's discussion on science legislation presented in October 1945 before the Senate Military Affairs and Commerce committees. The third chapter, entitled *World Control of Atomic Energy*, is the combination of an article under the same title published by Dr. Langmuir in *Proceedings of the American Philosophical Society*, January 1946, and his article "My Trip to Russia" (*Chem. Eng. News*, March 1946) describing his visit to Moscow in June 1945 to celebrate the "220th anniversary of the founding of the Academy of Sciences of USSR" (known as Russian Imperial Academy of Sciences from 1725 to 1917).

The fourth chapter of the book is somewhat more technical, and deals with Willard Gibbs' equation

$$\frac{dF}{d(\ln p)} = \sigma kT,$$

where p is the partial pressure of the adsorbed substance in equilibrium with the surface of the liquid, σ is the number of molecules adsorbed on the surface per unit area, T is the absolute temperature, and k is the Boltzmann constant. F , which may be called the spreading force, is given by:

$$F = \gamma_0 - \gamma$$

where γ_0 is the surface tension of the pure solvent, and γ is the surface tension of the solution.

The remaining fourteen chapters consist of the important publications of Dr. Langmuir on such subjects as Constitution of Liquids; Distribution and Orientation of Molecules; Atomic Hydrogen as an Aid to Industrial Research; Flames of Atomic Hydrogen; Forces Near the Surfaces of Molecules; Isomorphism, Isosterism and Covulence; Metastable Atoms; Condensation and Evaporation; Mobility of Cesium Atoms Adsorbed on Tungsten; and, last but not least, the Types of Valence.

In particular, in chapter fifteen Dr. Langmuir discusses the recent paper of R. W. Wood (*Phil. Mag.*, 1915, 30, 300) concerning a stream of mercury atoms impinging on a plate of glass held at a definite temperature, and proposes a new interpretation of Wood's results which is more in harmony with other facts than that suggested by Wood himself.

The volume is supplemented by ten pages of bibliography, consisting of a complete list of papers published by Irving Langmuir arranged according to subject matter.

The price of this elegant volume is somewhat high (\$10.00), but the book is certainly worth it for those scientists who are working in the institutions whose libraries do not possess chemical periodicals dating as far back as 1909.

G. GAMOW

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PARADOX

The Science of Chance. Horace C. Levinson. 347 pp. Illus. \$3.00. Rinehart. New York.

IT IS both easy and pleasant to characterize this book, which is a revision of the author's previous *Your Chance to Win*. It is written on a topic which is of broad interest and importance. The style is clear and lively. The treatment, considering the complexities and pitfalls of the subject, is remarkably accurate. In short, it is a good book.

More and more widely do we now recognize what the present reviewer, writing in this same journal twenty years ago, called "The Reign of Probability." Business, war, survival, the turn of a die, the motion of an electron, judgments in cases where we do not have sufficient knowledge to draw a strict conclusion by other means—all such, and many more, phenomena are controlled or guided by probability. It is of first-rate importance that generally intelligent persons have an increased knowledge of the theory of probability: that they understand the nature of the great central paradox that chance, which sounds as though it ought to be the antithesis of all regularity, is subject to laws.

Approximately the first two hundred pages of Dr. Levinson's book are devoted to the theory of probability. To make it interesting and to furnish simple and readily understandable examples, his illustrations are almost wholly drawn from games—coin-tossing, roulette, craps, and bridge. This preoccupation with games and gambling, even if faintly disreputable, is amusing and instructive. It means, however, that he does not have the space for treatment of the role which probability plays

in modern science (atomic physics and genetics, for example), in theories of evidence, and indeed in the whole theory of knowledge. Thus the emphasis on games does tend to obscure the broader and more serious side of the subject.

Granting the choice of material, the portion of the book on probability is generally excellent. The author does, to be sure, seem to have somewhat overconfident ideas on the matter of "proving" or "disproving" a probability statement; he is somewhat sketchy (but perhaps appropriately so) on the relation between a mathematical probability and a statistical probability. But he succeeds in getting across a good many of the important ideas in a clear and attractive way.

The final hundred pages on statistics are slightly less satisfying, being for the most part devoted to explaining topics which would be included in the most elementary course on applied statistics. Again, this may be the proper choice for the audience Dr. Levinson seeks. But it would seem both possible and desirable to give, even to a very general audience, some idea as to such topics as quality control, design of experiments, theory of inference, sequential sampling, etc.

WARREN WEAVER

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SEMANTIC ORIENTATION

Science and the Goals of Man. Anatol Rapoport. 262 pp. \$3.50. Harper. New York.

WETHER used for communicative, heuristic, or expressive purposes, language is one of the basic tools governing relationships between men and the control of man over his world. It is through language as a system of symbols that man constructs a psychological map of his physical world. The value his map has for understanding, controlling, and making predictions about the physical world depends upon the care with which it is constructed and used.

The best aid man has fashioned to construct accurate symbolic maps is the scientific method, for as far as we know this is the only method making possible the creation of "symbolic maps whose structure resembles as closely as possible the structure of reality." It is through the use of such maps that we gain the ability to manipulate our environment (including ourselves) and to make predictions about that environment—and to the extent that we master this technique to that extent we become masters rather than victims of our circumstances.

The purpose of this book by Dr. Rapoport, a professor of mathematical biology, is to provide a "semantic orientation" by means of which more accurate symbolic maps are constructed and to indicate that the use of such maps (and their construction) must not be limited to the study of the physical world. These maps must be related to all areas of human experience and interpersonal relationships. From this it follows that a scientist must, in order to be consistent, "subscribe to certain values (and discard others) not because he is a 'good citizen' or a product of Neptunian culture, or a

member of the X-ist church, but because he is a scientist." Being a scientist implies certain kinds of communication, orientation, and values—three aspects of human life with which the book is concerned.

The consequence of being a scientist in all behavior rather than in just certain specified areas or at certain times means that there exist no taboo topics outside the realm of free inquiry, that nothing is sacred. A critical attitude must be maintained toward all formulations of knowledge. For the scientist there is "no last word but only the latest," as Wendell Johnson puts it in his book *People in Quandaries*, and there can be no oracles, no authority as such.

This morality of the scientist demands that he cannot be impartial in the choice "between a procedure based on misvaluation and superstition and one based on accurate observation, critical attitude, and carefully weighed arguments. Similarly, he cannot be impartial in his judgments about forms of social organization or about patterns of culture, if it is clear that one form tends to encourage scientific behavior and another to inhibit it." This morality prescribes a loyalty for the scientist, namely, to a social order which *does not* demand loyalty *a priori*, one in which investigation, evaluation, criticism, freedom of inquiry and intellectual cross-fertilization must be possible and welcomed.

This makes science not merely a tool, but a way of life. This is scientific morality. This is the way of freedom and sanity. Thus, science determines for itself the ultimate ends for which it can and should be used.

Dr. Rapoport's book is written in direct and untechnical language, illuminated by simple and provocative examples, and may be recommended for profitable reading to the general reader. The book should serve to lighten the inhibiting load of those burdened with the "original sin" (from the scientist's point of view) of the fear of inquiry and to provide courage to follow wherever scientific inquiry leads.

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FOR INVENTORS AND OTHERS

Patent Tactics and Law (3rd ed.). Roger Sherman Hoar. xv + 352 pp. \$7.00. Ronald Press. New York.

THE author, commercial attorney of the Bucyrus-Erie Company, states in his preface that it has been his aim completely to revise the 1926 and 1939 editions to meet the revolutionary changes that have taken place in patent law. The revised *Rules of Practice of the Patent Office*, the new official *Manual of Patent Examining Procedure*, and the important cases of recent years have made the need for revision particularly urgent.

The volume is easy to read, written in a popular, nontechnical style in order to make the subject comprehensible to business executives, industrial engineers, inventors, and patent attorneys alike. The informality of presentation does not, however, detract from its excellent organization in short, well-indexed chapters, con-

taining many practical suggestions with graphic illustrations. It can be used for reference in respect to specific patent problems, or for reading in its entirety as a general introduction to the subject. There is an excellent glossary of terms relating to patent law, and the chapters are well documented, providing for further study on any particular phase of patent law.

The text clearly explains what a patent is, who is entitled to a patent, what is patentable, searches and procedure in the Patent Office, and the drafting of papers, and there is a discussion of patent law dealing with such matters as interferences, infringements, and appeals. There are interesting comments on understanding the mental processes of patent examiners and the courts. The chapter on the business aspect of patents contains much useful material on the evaluation of patents and patent licenses in accordance with proper accounting practice. The author expresses the opinion that the chapter on anticipation and domination is the most important in the entire book. It is exceptionally well written and carefully explains and compares those important matters. There are good practical suggestions for organizing a patent department in a corporation, choosing an attorney, the essentials of a workable patent law library, and forms for patent contracts. Although one could hardly expect an exhaustive treatment of the interpretation and validity of patents, or a detailed discussion of foreign patents, within the limits of a single volume of such wide scope, it is regrettable that the author did not draw further from his wide experience to enlarge upon these important fields.

It may be noted that it is somewhat strange to find individuals such as Wendell Berge and Thurman Arnold put in the category of "enemies of free enterprise" when their consistent effort over the years has been to preserve free enterprise in this country against monopoly and against the elimination of competition on all fronts, including those situations in which the owners of patents have attempted to stretch their patent rights beyond their legitimate scope, and in which it has become necessary not to attack the patent system but to make a counterattack in order to meet the attack being made by the proponents of the patent system.

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THE MACHINE AND HOMO SAPIENS

Giant Brains, or Machines that Think Edmund C. Berkeley. xvi + 270 pp. Illus. Wiley. New York. Chapman & Hall. London.

THE author is president of E. C. Berkeley and Associates, and was chief research consultant for the Prudential Insurance Company after the war, taking part in the company's studies on the large-scale automatic computer UNIVAC. His subsequent experience since 1948 has been in consulting on automatic machinery for computing and handling data. So he is fully competent to write a book on these "giant brains." His account is extraordinarily clear and readable, and nearly

all of it is intelligible to a layman (like the reviewer). The diagrams are very well done. The emphasis is on the basic facts of "the principles of thinking, of mathematics, of science, of engineering, etc., " that will probably still be useful in the design of new machines to supplement some of the present giants, already obsolescent.

Do these machines think? If they do not, they give an uncanny parody of thinking and memory—"stored information"—on a rather low level. Until more is known about what human thinking and human memory are, the question must remain unanswered. The fact so far seems to be that *Homo Sap*, in spite of his difficulty in adding 2 and 2 to get the right result, whereas a machine can do—among other things—5,000 additions accurately in one second, is still master. *Sap* sets the machine its problems and tells it how to solve them.

Among the topics explained are punch-card machines, differential analyzers for solving certain types of differential equations, the electronic numerical integrator and calculator, and the logical-truth calculator. For the last there is a clear presentation of the underlying mathematical logic. The binary system of writing numbers is explained in connection with some types of calculators. The school algebras of about fifty years ago carried a chapter on this and other "scales of notation." It was dropped because there seemed to be no use for it.

The general reader may find it interesting and profitable to read Chapters 11 and 12 and Supplement 1 first. These discuss what machines might do for men, if men had sense enough to let them do it, types of problems, and the social control of thinking machines and the obstacles to such control.

Finally, there is an extensive selected bibliography, with many items of interest to the layman.

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BRITAIN OF THE SOUTH

The Invasion of New Zealand by People, Plants and Animals. Andrew Hill Clark. xiv + 465 pp. Illus. \$6.00. Rutgers University Press, New Brunswick, N. J.

THIS book is the first comprehensive study of the physical alteration by European man and his "satellites" of the world's most recently occupied temperate land mass. Coverage is restricted to the South Island of New Zealand, the larger but more sparsely populated part of the Dominion. The area of the South Island is approximately that of the state of Illinois, the climate and present rural landscape somewhat comparable with those of Great Britain, though differing in having a far more diverse topography and a consequent high local variability of precipitation. A further pronounced difference is that New Zealand soils are immature and of a prevailingly low natural fertility, although this has the somewhat anomalous concomitant that many introduced organisms, such as California pines, grow at rates far more rapid than in the countries of their origin.

The author, whose experience has included two years' residence at a notable center of geographic study, Can-

terbury College, Christchurch, has divided his book into five main sections dealing, respectively, with the land, the people, the invading animals, the invading plants, and a general conclusion. To these are appended data on regions and types of farming, exhaustive reference notes to the fourteen chapters, and a bibliography covering more than twenty pages. The latter is extraordinarily comprehensive, including numerous obscure sources of information. It lacks references, nevertheless, to a few works which would seem particularly apt, such as Basil Howard's *Rakiura*, published in New Zealand in 1940 for the Stewart Island Centennial Committee. Howard's treatment of the early voyagers to the Foveaux Strait area, and his account of the sealing and whaling period, are almost equally up Professor Clark's South Island alley. Howard's bibliography also lists other general works pertaining to the South Island that have apparently not been used by Clark.

The book opens with an account of the primitive habitat, both physiographic and biotic. This is brief but adequate, if allowances are made for the author's lack of familiarity with the endemic flora and fauna as indicated by incorrect groupings, misspelling of generic names, and certain other misunderstandings. In the next chapter, relating to the Maori aborigines and early European contacts, he hits his stride and provides us with one of the best summaries published to date. This is followed by discussions of the flow of settlement after 1840, and the changing population in relation to numbers, distribution, and character. Convincing use is here made of statistical data unearthed from many sources. Some of these tend to dispel illusions that have become fixed not only in New Zealand but also throughout wide geographical circles. The author recounts the effect of the Napoleonic wars and the resulting poverty in the British Isles upon early emigration to New Zealand. The population is overwhelmingly of English, Scotch, and Irish stock, but Clark points out, perhaps for the first time, the relatively large percentage of people that reached New Zealand by way of Australia and Tasmania. Both the gold diggers and the preconditioned sheep farmers arrived largely across the Tasman Sea, the Australian increment accounting for more than one third of the net immigration in the decade following 1860. In 1921 only 14 per cent of New Zealanders sixty or more years of age had been born within the country. Historians in New Zealand, according to Clark, have exhibited too strong a tendency toward "Little-Englandism." He criticizes the cult of ancestor worship, which traces virtually all the blood lines to a hand-picked selection of the best of all classes in England and Scotland. As a matter of fact, he concludes, the colonists came largely from the class of "respectable poor," bettering themselves very rapidly in New Zealand and building the present nation which is so highly characterized by energy, independence, and self-reliance.

The reviewer's personal experience in New Zealand would lead him rather to emphasize another trait, which might be called the very opposite of ancestor worship. It is well expressed by Guthrie-Smith in his magnificent *Tutira* (1921). This author writes: "New

Zealand, if unlikely to produce a world poet or a world musician—brains do not emigrate, no intellect of the highest order has yet arisen anywhere outside Europe—can lay claim, in her founders, to courage and character; in her present population, to the saving virtue of simplicity." Surely such a point of view is all too modest and deprecatory to come out of the land that produced Lord Rutherford and Katherine Mansfield. It has been said in many intellectual circles, indeed, that New Zealand's most important export—to her detriment—has been brains!

The South Island is sheep country par excellence, supporting fourteen million of the creatures, but Clark has performed a geographical service in showing that it was not "foreordained" as such. Other destinies are thinkable. If the Japanese, he suggests, had begun their ill-fated expansion a century earlier and had succeeded in populating New Zealand, the products of the land, and the face of nature as altered by man, would doubtless have been almost unimaginably different from what they have become in the "Britain of the South."

Despite spectacular evidence of success, this island yet shows the unfavorable influence of prevailingly urban immigrants who looked on land merely as a commodity, as a means of earning a living. They had little of the peasantlike feeling of love of land and countryside, writes Clark, which was so characteristic of the Pennsylvania Germans and certain other continental Europeans who settled in the United States, and who have prospered without exhausting or seriously mutilating the areas in which they have continued to live and work.

The demographic and other human aspects of this book are succeeded by long and illuminating chapters on sheep and cattle, the less important domestic animals, the introduced pests (especially rabbits and deer), the invading culture plants, and the exotic trees from Europe, North America, and Australia, which have so radically altered both landscape and economy.

By and large this is a scholarly and indispensable work, as readable in most parts as conciseness and quantitative treatment permit, and shot through with rewarding flashes of geographical wisdom and touches of truthful whimsicality.

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WADERS

The Sandhill Cranes. Lawrence H. Walkinshaw. x + 202 pp. Illus. Cranbrook Institute of Science. Bloomfield Hills, Mich.

FOR fifteen years Walkinshaw has studied the four races of the sandhill crane (*Grus canadensis*) throughout its range from the tundra of Alaska to the tropical savannas of Cuba and the Isle of Pines. He has also examined most of the skins and eggs of the species preserved in our various museums, and has made

himself familiar with the published observations and data of others who have written of this bird. This book is the result. The amassed material is well handled and the information, surprisingly extensive in many particulars, is given under convenient headings, such as plumages and molts, voice, locomotion, food and feeding habits, territorial and mating relations, courtship behavior, nesting, the eggs (description, number, incubation period), care and growth of the young, migration, and the known history of the crane populations of various sections of the country. A special appendix gives all the actual distribution records on which are based the author's statements of the ranges of the component races of the species in summer and in winter. There are also a good bibliography and an adequate index.

The illustrations are all from photographs and give a good idea of the habitat and the appearance of the cranes in different stages and parts of their lives. The book raises no questions and therefore does not attempt to supply answers. Its purpose is merely to present what is known of the sandhill crane, and this it does very well.

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BRIEFS

The Science of Flight. O. G. Sutton. 208 pp. Illus. 35. Penguin Books. Harmondsworth, Middlesex, Eng.

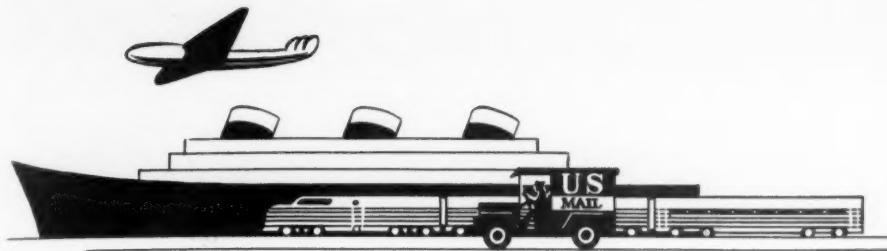
FIRST published in 1949, this Pelican Book is a semitechnical account of aerodynamics. It traces the story of the conquest of the air and the unfolding of the secret of flight, from the spears and arrows of primitive man to today's supersonic aircraft and the giant rockets.

Birds of America (Popular ed.). John James Audubon. 320 pp. Illus. \$2.95. Macmillan. New York.

LUDLOW GRISCOM, chairman of the board of the National Audubon Society, has written a 15-page introduction for the 288 full-page, 4-color plates of Audubon's best paintings. The various birds are grouped together in the families in which they belong, and a brief descriptive paragraph accompanies each plate.

Jane's Fighting Ships, 1949-1950 (51st ed.). F. E. McMurtrie, Ed. 50 + 416 pp. Illus. \$16.50. McGraw-Hill. New York.

MORE than 500 new illustrations have been added to the fifty-first edition of this naval annual. Flags, uniform insignia, 1949 strength of fleet, principal guns, naval appropriations, construction notes, aircraft, even data on guided missiles, for each of the forty-four nations having navies, are listed. A Japanese section makes its first appearance since the war. Mercantile marine figures for the various countries have been brought up to date.



CORRESPONDENCE

ACTION RESEARCH

As a member of the American Association for the Advancement of Science and reader of **THE SCIENTIFIC MONTHLY** for many years, I noticed the article in the January 1950 issue entitled "Action Research Among American Indians," by Laura Thompson. Because of the emphasis given in your magazine to precision of wording on the origins of pioneering in scientific ideas, I prompted to write to you. In the above-mentioned article the author describes on page 34 a new type of operative social investigation, "action research," or rational research. She gives credit for the development of these action methods in the social sciences to Collier and the late Kurt Lewin, without mentioning their originator. This may have occurred in the haste of writing any report. Without denying the efforts that have been made by Collier and Lewin toward the development and promotion of such research, it should be pointed out, for the benefit of all students and researchers who might need the guidance of proper references, that it was J. L. Moreno, author of *Who Shall Survive? A New Approach to the Problem of Human Interrelations* (1934), who for many years before them developed the theoretic foundations and instruments of action research. Terms such as "action test," "action methods," "science of action," and "action research" were used by him in this context. Thirteen volumes of the journal *Sociometry, A Journal of Inter-Personal Relations*, between 1937 and 1950 document this.

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ALL social science workers are in debt to Dr. Moreno, and most of them acknowledge the indebtedness, as certainly I do. But to claim that Dr. Moreno initiated that complex of operations now known as action research is to disregard what that complex of operations is; and to claim that he initiated that component of action research known as layman participation is to ignore social research history antedating Dr. Moreno's earliest work by decades, and longer. Space limitations excluded from my article as published in **THE SCIENTIFIC MONTHLY** the footnotes and bibliography ac-

companying the paper; hence, a few words in supplement are appropriate here.

1. Fully developed action research as now understood and sometimes practiced consists in the identifying, within a problematic situation, of a significant problem of practical importance; of the attack on this problem by a team of specialists from the several disciplines needed to solve the problem; of the formulation, through the findings, of a hypothesis of needed useful change; of the implementation of that change; and of the analysis and measurement of the consequences, again by the several technicians working as a team (since the consequences, just as the factors initially investigated, require multiple-disciplinary study being, for example, biological, psychological, economic, administrative, etc.). The sequence of operations is stated by Kurt Lewin in his *Resolving Social Conflicts* (Harper, 1948, 205, *et seq.*). The multiple-discipline but integrative nature of the methodology is stated by H. A. C. Dobbs in his *Operational Research and Action Research* (Institute of Ethnic Affairs: Washington, 1947) and by John Collier in The United States Indian Administration as a Laboratory of Ethnic Relations (*Social Res.*, 1945, 12, 265-303).

2. Layman participation adds both practicality and profundity to the action research process. Layman participation is essential to, and formally recognized in, Dr. Moreno's research: obviously, for example, in his psychodrama, but also explicitly in his sociometry. To quote Helen H. Jennings, for example (*Sociometry in Group Relations*, American Council on Education: Washington, 1948, 12, 13): "Students [being sociometrically tested] must fully understand that the question is asked in order to be useful to them and that their personal reaction is important if they are to benefit from the consequence. . . . [And] it must be possible to carry out the original agreement with them on the basis of which they made their [preferences] choices."

However, this essential component of user-participation, lay-participation in social research is not new but very old. It is of the essence of the social survey designed to find facts and through the finding process to involve the citizenship in action on the facts. That

phase dates back to 1910 at the latest (the Pittsburgh Survey). But the bibliography of my paper included an article (What is Cooperative Action Research? *Newsletter of the Institute of Ethnic Affairs*. Washington, Feb. 1946) which describes a famous case one century old—that of the Rochdale spinners, laymen all, whose research conducted as a group operation discovered the principle of consumer cooperation and invented the mechanisms which are valid even today, and experimentally instituted the social change.

Actually, the fully developed action research enterprise of today is a utilization of principles and methodologies as old as science, but focused and integrated toward and through social action, and a utilization and self-utilization of the layman in the research process, older than the example of the Rochdale spinners; as old, in fact, as human society.

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PERVERSION OF VALUES

What are we then? the lords of space?
The Master-mind whose tasks you do?
Jockey who rides you in the race?
Or are we atoms whirled apace,
Shaped and controlled by you?

—“Prayer to the Dynamo” in
Henry Adams, *Prayer to the
Virgin of Chartres*.

EVERY age has its own moral base, and the perversion of it; many of us are unable to distinguish between the two. Centuries ago certain groups in Palestine had lost the spirit on which true Judaism was built to the extent that form and ritual were the basis for morality. They were called down by an itinerant rabbi, when they protested abuse of the Sabbath, with “The Sabbath was made for man, and not man for the Sabbath.” It is time now to protest the attempts of Elgin Williams to make man an insignificant part in a system of morals created by the “logic of the machine.”

We must have peace, tolerance, and abundance *to have machines!* There must be free inquiry, liberty of expression, and respect for the dignity of the individual *to have machines!* The creative principle is to make machines “to make more machines to make more machines!” Man has been completely lost, and what is substituted is a pattern of thought which Mr. Williams claims is dictated by machines men have made. Other

things men have made he chooses to neglect as unimportant. “While creating new and better means [of mastering nature, he [modern man generally] has become enmeshed in a network of those means and has the vision of the end which alone gives them significance—*man himself*” (Eric Fromm. *Man For Himself*. New York: Rinehart, 1947).

It is easy to protest the “morals of capitalism” as basis for our society; Mr. Williams has done it better than many. However, for the perversion of a system which men thought gave opportunity for development of mankind (free individual enterprise), it is not proper to substitute the perversion of another system which men think gives opportunity for development of man (modern science and technology). The end—development of man—must not be lost unless we really are willing to make man’s development a mere by-product of things man has created.

Great thinkers on social problems have always begun with man and his possibilities. Aristotle, St. Thomas, Hobbes, Bentham, Dewey, or anyone you wish, are not dissimilar in where they begin, but in the potentialities they see in man. Today we need new ontologies constructed in light of what science now tells us about man; on these we can build new ethical systems. But it is as disastrous to build on machines, or a capitalist system, or a socialistic system, as it has been to build on national socialism or communism. Man will not be lost.

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THEY'LL GIVE A LOVELY LIGHT

(*The United States Public Health Service is spraying mosquitoes with chemicals that make them glow in the dark, to discover how far they fly at night and where.*—N. Y. Times.)

How aggravated the mosquito
Must be to lose his incognito!
It's obvious that when he's lit
On darksome nights, he will get hit.
Our rescued pores of epidermis
Will bless the U. S. Public Service
And as mosquitoes fluoresce
We'll slap, then cheer for P. H. S.

LOUISE DYER HARRIS

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